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MEMOIRS
OF THE
Royal Society;

Being a New ABRIDGMENT of the

Philosophical Transactions:

Giving an ACCOUNT of the Undertakings, Studies, and Labours of the LEARNED and INGENIOUS in many considerable Parts of the WORLD; from the first Institution of that ILLUSTRIOUS SOCIETY in the Year 1665, to the Year of our LORD 1735 inclusive.

The whole carefully abridg'd from the Originals, and the Order of Time regularly observ'd, with a Translation of the LATIN TRACTS, and the Theoretical Parts apply'd to Practical Uses; also an Explanation of the Terms of ART as they occur in the Course of the Work. Being a Work of general Use to the Publick and worthy the Perusal of all MATHEMATICIANS, ARTIFICERS, TRADESMEN, &c. for their Improvement in various Branches of Business.

By Mr. BADDAM.

VOL. VII.

Illustrated with a great Variety of COPPER PLATES.

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M,DCC,XL.

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MEMOIRS
OF THE
ROYAL SOCIETY;
Being a New ABRIDGEMENT of the
PHILOSOPHICAL TRANSACTIONS.

An Account of the Phænomena *of a very extraordinary* Aurora Borealis *ſeen at* London; *by Dr.* Halley. Phil. Tranſ. N° 363. p. 1099.

ON *Tueſday Nov.* 10. 1719 about five o'clock in the morning, Dr. *Halley* eſpied certain white ſtreaks in the ſky, that ſeemed nearly perpendicular; and which, whilſt he conſidered them, appeared inſtantly to vaniſh, and ſoon after others as inſtantaneouſly to ſucceed them: He began to imagine, that this probably was to be ſome part of the phænomena of the *Aurora borealis*, only there appeared nothing like that luminous arch, which was ſo often ſeen in the north; till looking up towards the zenith, he eſpied an entire canopy of ſuch kind of white *ſtriæ*, which ſeemed to deſcend from a white circle of faint clouds, about 7 or 8 degrees in diameter, which circle would ſometimes vaniſh on a ſudden, and as ſuddenly appear again: The Dr. obſerv'd, that the center of this place of concourſe was not preciſely in the zenith, but

rather 14 degrees to the ſouthward thereof; which he was well enabled to eſtimate by a ſtar, that on each return thereof ſhewed itſelf about the center of the circle: This ſtar is the 33d of the great bear in *Tycho*'s catalogue, whoſe diſtance from the pole at that time was 52° and ½, and which about ½ an hour after five that morning paſs'd the meridian; ſo that thoſe rays centered very nearly on the meridian itſelf: It was a very agreeable ſight, till ſuch time as day-break began to obſcure theſe lights, which were but faint tho' ſufficiently diſtinguiſhable; none of them came lower than to about 30 or 40° of altitude, and they ſeemed not to have aſcended from the horizon. The ſky was perfectly ſerene and calm; which ſeems to be one of the concomitant circumſtances, attending the *Aurora borealis*, of which this was certainly a ſpecies; for, the night following a ſtrange ſtreaming of light was ſeen in the air, which the Dr. obſerved from ½ an hour after 9 till 11 o'clock, when there came ſo thick a fog as to put an end to his proſpect: But during all that time there aſcended out of the E. N. E. and N. E. a continued ſucceſſion of whitiſh *ſtriæ*, ariſing from below; and after changing into a ſort of luminous ſmoke, paſſed over head with an incredible ſwiftneſs, not inferior to that of lightning; and in ſome part of its paſſage ſeemed gilded, as it were, or rather as if the ſmoke had been ſtrongly illuminated by a blaze of fire below: Some of the *ſtriæ* would begin high in the air; and a whole ſet of them ſubordinate to each other, like organ-pipes, would preſent themſelves with more rapidity than if a curtain had been drawn from before them; ſome of which would die away where they firſt appeared, and others change into a luminous ſmoke, and paſs on to the weſtward with an incredible ſwiftneſs; and the Dr. is of opinion, that had it not been for the moon, then 10 days old and very bright, this, for the time, would have been as conſiderable an appearance, as that of the 6. of *March*, 1716.

An Account of the ſame Appearance, ſeen at Cruwys Morchard *in* Devonſhire; *by Mr.* Maunder. Phil. Tranſ. N° 363. p. 1101.

ON the 26. of *October* 1719, between 7 and 8 o'clock in the evening Mr. *Maunder* obſerved ſome ſmall appearance of a ſurpriſing light (reſembling thoſe deſcribed in a former *Tranſaction* and ſeen on *March* the 6. 1716.) *viz.* 3 or 4 conſiderable coruſcations in form of pyramids, of a reddiſh colour inclining to yellow, which roſe about 50 degrees above the horizon, and continued but a few minutes: But the north part of the hemiſphere

ſphere was very bright and red all the evening, both before and after till 10 o'clock, if not longer.

On the 10. of *November* theſe lights were ſeen again about four in the morning, of which ſome ſaid, that the element open'd (as they termed it) ſometimes at one place, and ſometimes at another; from whence iſſued great ſhining lights that continued a while, and then vaniſhed by degrees, and the holes cloſed up again: This continued till day-break.

The following evening Mr. *Maunder* coming from *Tiverton* about ½ an hour after eight, he obſerved the north part of the horizon very bright and reddiſh (notwithſtanding the moon being about 10 days old was then in or paſt the meridian, and ſhone very bright) in a ſhort time the ſtreaming luminous rays began to appear very plainly, ſome in one ſhape, and ſome in another; ſeveral of them reſembling cones or pyramids, but moſt of them ill terminated; ſome of which mounted very high, almoſt to the zenith, to which place or near it, they all or moſt of them ſeemed to point: Soon after there appeared a long ſtreak about 30 degrees, parallel to the horizon, and about 15 or 20 degrees diſtant therefrom and about two or three degrees broad; but ill terminated and of a fiery red colour, which emitted ſome of the ſame ſtreaming beams towards the zenith: About 6 or 7 minutes after there appeared (ſomewhat ſuddenly) a circular figure like an *Iris*, but twice as broad, and of a pale colour: The eaſt part was terminated by the horizon at full eaſt, if not ſomewhat to the ſouth, and the weſt end about north-weſt; the upper part of its arch being 50 or 60 degrees high, a great many luminous rays darted therefrom upwards and downwards (or elſe croſſing it from the horizon) at oblique angles pointing to the zenith, eſpecially, from the north eaſt part: This continued, as near as Mr. *Maunder* could conjecture, about 8 or 9 minutes, when it divided and diſappeared: After an interval of 3 or 4 minutes, another *Iris*-like figure appeared (of a colour, as it ſeemed, paler than any of the ſtreaming lights had been) whoſe diameter was leſs than that of the former, and ſhewed more than a ſemicircle above the horizon, the upper part of its arch approaching near the zenith; he could not obſerve any rays to paſs from, or a-croſs this, as from the other; the center of this laſt was much more to the weſt than that of the firſt: After continuing about a minute or two, it began to break in the upper part of its arch, and ſhining particles, emitted from both its broken ends towards the zenith (to which they were near before) or rather a little beyond it to the ſouth or ſouthweſt, there formed a ſort of *corona*, incurvated

vated ſomewhat like flames reverberated on the arch of an oven: It ſeemed to Mr. *Maunder* and others to be finely tinged with various colours, red, yellow and blue, *&c.* and emitted every way, excepting ſouth and ſouth-weſt, long flame-coloured rays: After this had continued about two minutes, its ſhining light abated, and it left behind it for ſome minutes ſomething like a whitiſh light, and in colour reſembling that which the light on the 19. of *March* 1719 left behind it, after the fiery particles were extinguiſhed, but thinner.

It is to be obſerved, that all this while the moon ſhone very bright, from which this *corona* was not very far diſtant, perhaps not 20 degrees to the north-eaſt: After this there continued to be emitted ſeveral fiery coloured or yellowiſh ſtreaming lights, ſometimes more, ſometimes leſs, now here, now there, all along the north part of the hemiſphere; but moſtly from the north north-eaſt: All this while ſomething like ſmall whitiſh clouds (which appeared to Mr. *Maunder* to move towards the zenith, or to point a little more ſouthward, but diſappeared as they approached the moon) were driven very ſwiftly and at very ſhort intervals, moſtly coming from the eaſt and north-eaſt, but ſeveral likewiſe from north and north-weſt: Mr. *Maunder* took but little notice of this at firſt; ſuppoſing it had been nothing but the reflection of the other lights, or the ſhadows of the clouds (which the north parts were pretty full of) as the ſtreams of light paſſed behind them; but at laſt he obſerved, that when the lights abated at any time, this kind of clouds continued to fly as ſwift and frequent as ever. This he obſerved till 12 or 1 o'clock next morning; ſeveral others ſaw it next morning till almoſt break of day, when it appeared much more red and fiery than it was in the evening, the moon being then, probably, ſet: Some people obſerved tall cones to ariſe in the eaſt, and to be carried to the weſt pretty ſwiftly in an erect poſition.

A farther Account of the ſame appearance, as ſeen at Dublin *in* Ireland. Phil. Tranſ. N° 363. p. 1104.

ON the 10. of *November*, 1719. at *Dublin* there were ſurpriſing lights obſerved in the northern ſemicircle of the horizon: The afternoon was very calm and ſerene; about ſix o'clock in the evening the ſky was tinged with a ſtrange kind of light, and ſome ſtreams began to project from the north and north-eaſt; one of them aroſe about north by eaſt, and was nearly a ſubtenſe of an arch between that and ſouth weſt by weſt; it was ſomewhat incurvated towards the ſun; and what was ſeen

of

of it (for, the north part of the horizon was concealed by houſes) very much reſembled the tail of a comet: About the ſame time there was one or two that aroſe in the eaſt, aſcending obliquely, ſo as to leave the zenith ſeveral degrees to the northward.

Theſe *ſtriæ* continued to appear and diſappear alternately till towards 8 o'clock in the evening; they were pyramidal, and their *vertices* frequently projected ſeveral degrees to the ſouth of the zenith.

Between 9 and 10 o'clock this Gentleman was agreeably ſurpriſed with a kind of coruſcation or flaſhing, that ſhewed itſelf between 20 and 60 degrees from the zenith, in the ſouth or ſouth by weſt; and which from 4 or 5 and ſometimes from more places at once, darted with a velocity not much inferior to that of lightning; and by interfering with each other produced a beautiful tremor or undulation in that ſubtile vapour, which reſembled the beams of the ſun, reflected on a cieling from the ſurfaces of two or three baſons of water: Theſe undulations of light were only viſible at the inſtant of coruſcation, and were of a pale whitiſh colour, ſomewhat reſembling the flaſhes, produced by the violent agitation of mercury in an exhauſted receiver; but ſo ſtrong, that a Gentleman, who about that time happened to be in a room without a candle, took it for common lightning: Thus it continued inceſſantly for more than an hour; during which time ſeveral lucid area's, like ſmall clouds, diſcovered themſelves in the pure ſky; and after they had continued about five or ſix ſeconds of time, as near as could be conjectured, would inſtantaneouſly diſappear; moſt of them pretty much reſembled a very thin white ſmoke or vapour illuminated by the full moon.

About ¾ after 10 o'clock, this vapour was almoſt ſpent, or by a briſk gale at ſouth by weſt diſſipated and driven to the northward; at which time between the weſt and north a vaſt body thereof, reſembling a very bright flame-coloured *crepuſculum*, ſeemed to be fixed: From this baſis ſeveral beams or *ſtriæ* of ſhining matter were emitted at uncertain intervals; and tho' it was not ſo ſenſible to the eaſtward of the north, yet ſeveral huge pillars were alſo emitted from thence; one of which aroſe directly under the pole, and both as to its bulk and denſity ſo far exceeded all others that had preceeded it, that the ſmalleſt print might have been read by the light thereof, had not that of the moon (which ſhone very bright) pretty much effaced it: It was tinged with a kind of yellow and violet colour: In about two or

three

three minutes time it died away, and was ſucceeded by others of an inferior order. It was now about ¼ after 11 o'clock, when nothing but repeated phaſes of the ſame appearance preſented themſelves to view; the vibrating motion had ceaſed; the vapour ſhewed itſelf no longer in lucid area's; the ſtreams of light were not ſo frequent, and thoſe more languid than before; and the bright *aurora* having ſettled near the horizon, this Gentleman concluded the ſcene was at an end, and accordingly gave over the queſt of new phænomena, only he obſerved, that about N. E. there appeared ſome clouds that reflected an unuſual kind of reddiſh light.

On the 24. of *Nov.* the ſame phænomena were repeated, tho' not with the ſame variety: About ¼ after 10 o'clock at night, a vaſt body of ſhining matter was collected between N. W. by W. and N. by E. in the form of a ſegment of a circle, whoſe center was about 25 or 30 degrees below the horizon; from its periphery a few ſhort pyramidal ſtreams of the ſame luminous vapour aſcended by a ſlow and nearly uniform motion, and were exceeding rare, ſo as not to efface the ſmalleſt of the fixed ſtars; and in a minute or two vaniſhed: It was very remarkable, that the light which that collection of vapour emitted was ſo great, that in the otherways dark night, this Gentleman could thereby (at ¾ after 10 o'clock) read the title of the *Phil. Tranſ.* which then happened to lie on his deſk; and at four or five yards diſtance ſee the ſmalleſt books in his ſtudy.

An Account of another very conſiderable Aurora borealis *obſerved at* Sreatham *in* Surry; *by Mr.* Hearne. Phil. Tranſ. N° 363. p. 1107.

MR. *Hearne*, having ſeen Dr. *Halley*'s account of the coruſcations in the evening and morning of *November* 10. 1719. found the obſervations made on that appearance very agreeable to what he himſelf had obſerved the evening of that day, and to what he did not obſerve at that time, but had an opportunity of obſerving in the night of *December* 11. much more plainly than Dr. *Halley* had in the night of *November* 10.

December 11. about one o'clock at night (or rather in the morning of *December* 12.) he obſerved coruſcations of a much different colour, and in a very different manner from any he had ſeen before.

The ſtreams of light that darted upwards from the horizon ſeemed to be at a conſiderably greater diſtance; but not at all in leſs quantity than thoſe of *Nov.* 10. But their meeting

in a point near the zenith, and forming there a kind of canopy, was what was particularly remarkable in the manner of the coruſcations, now different from thoſe of *Nov.* 10.

The ſtreams of light roſe from the horizon only towards the north, and on each hand towards N. E. and N. W. but near the zenith a canopy was form'd of ſtreams of light, meeting in a point, not only from thoſe quarters, but alſo from the ſouth, *&c.* only they extended downwards from the zenith but a little way to thoſe points, and were neither in ſo great quantity, nor quite ſo bright as thoſe northwards: At firſt Mr. *Hearne* took the point in which the ſtreams met to be exactly the zenith; but upon obſerving it ſomething longer, he found it a few degrees to the ſouth of the zenith: The ſtreams of light near the zenith, that form'd this canopy, were of a pretty bright colour, and in great quantity darted very ſwiftly.

On each ſide of the north towards E. and W. (but not exactly in the north itſelf) from about 10 or 15 degrees to 40 or 50 above the horizon, the ſtreams were of a glowing red colour; whereas all he had ſeen before were very pale: The redneſs reſembled that of a burnt brick, and remain'd for a few minutes, like that track thro' which the meteor paſs'd in the ſpring.

The ſtreams appeared of this fierce colour, when Mr. *Hearne* firſt obſerv'd the coruſcations, and continued ſo for ſome time, till the redneſs wearing off by degrees, in about a quarter of an hour they appear'd of the uſual paleneſs, when he left them ſtill forming a canopy near the zenith, as above deſcribed.

The air was very calm and ſerene, not a breath of wind ſtirring; as it was likewiſe *Nov.* 10.

The moon was now a day or two older than it was *Nov.* 10. and a good deal farther to the weſt than when he obſerv'd the coruſcations that night, being then near full ſouth; ſhe had now round her what is commonly call'd a burr, and larger than ordinary, and ſeveral very lucid clouds at a little diſtance.

Aſtronomical Obſervations for the Year 1719. Phil. Tranſ. N° 363. p. 1109. *Tranſlated from the* Latin.

OCTOBER 10. 1718 in the morning *Jupiter* applied to the teleſcopic ſtars, whoſe places, Mr. *Pound* (on occaſion of the firſt appearance of the comet in 1680, of which *vide Phil. Tranſ.* N° 342) diligently enquired into, and which being verified he communicated to the Royal Society, together with

with an accurate obſervation of a near tranſit of *Jupiter* and again another *February* 11. immediately after the oppoſition of the ſun and *Jupiter*.

In the beginning of *Jan.* 1719 the places of the ſtars were as follows.

	Long.			N. Lat.				Long.			N. Lat.		
d	♌	29°	59′	43″	1°	7′	50″	*a*	♍	25′	41″	1° 28′	54″
e	♍	0	6	13	1	10	18	*x*	♍	5	43	0 51	56
c	♍	0	3	13	0	32	50						

Where it is to be obſerv'd, that the ſtars *d* and *e* have preciſely the ſame declination this century; and that *x* is a very ſmall ſtar, and for that reaſon omitted in the former deſcription.

On the 9. of *October*, 1718. 17h. 50′ equated time, *Jupiter*'s eaſtern limb touch'd the line that joins *e* and *c*; and at the ſame time his centre was diſtant from *e* 21′ 20″, and from *c* 16′ 25″, and from *d* 19′ 35″; the ſmall ſtar *x* being near *Jupiter*, was hid and obſcured by his light.

On the 11. of *December* 18h. 30′ equated time, *Saturn*'s centre was diſtant from *Bayer*'s μ in *Libra* 28′ 32″, and more northerly than that ſtar 4′ 31″: Hence Mr. *Pound* concluded, that *Saturn*'s place was ♎ 10° 41′ 10″, with 2° 16′ 43″ N. Lat.

On the 11. of *February*, 1719, 6h. 56′ and ½ equated time, the centre of *Jupiter* (then retrograde) was diſtant from the ſtar *d* 10′ 42″.

At. 6h.	58′ and ¼	The ſame center was diſtant from *e*	6′	7″
9h.	37 and ⅓	The diſtance again taken from *d*	10	9
9	43 and ½	Again from *e*	6	11
9	49 and ½	*Jupiter*'s centre was diſtant from *a*	25	21
9	58 and ½	From the ſmall ſtar *x*	24	38

About 7h. *Jupiter*'s eaſtern limb touch'd a line drawn thro' *x* and *e*; therefore, *Jupiter* was at that time in 6′ of ♍ with 1° 16′ 30″ N. Lat.

On the 13. of *February* 8h. equated time, the declination of *Jupiter*'s center, as meaſur'd by the micrometer, was more northerly than that of both the ſtars *d* and *e* 11′ 37″, and at 8h. 20′ the ſame difference was found 11′ 36″. But at 8h. 48′ *Jupiter*'s center was diſtant from *e* 17′ 40″.

On the 22. of *April* 10h. 45′ equated time, *Saturn*'s centre followed μ of *Libra* 4″ and ½ of time, or 1′ 8″ *R. Aſcenſ.*

But

But he was found by the micrometer to be more northerly than the fix'd ſtar 35′ 25″: But in the *Catal. Britan.* the ſtar was at that time in 10° 16′ 8″ ♏, and 2° 3′ 54″ N. Lat.

On the 16th of *May* 8h. eq. time *Jupiter* followed *Cor Leonis* 1° 34′ and ½ *R. Aſcenſ.* but was more northerly than that ſtar 41″ and ½ of time, that is, 10′ 7″ of an arch of the heavens.

The ſame night at 15h. 18′ app. time Mr. *Stephen Grey* obſerv'd, that *Mars*, as to *R. Aſcenſ.* followed the eaſtern ſtar in *Capricorn*'s tail 16′ 15″; and at the ſame time was but 11″ more ſoutherly than the ſtar.

On the 7th of *June* 10h. 15′ app. time, *Jupiter* being direct, return'd again to the above-mentioned teleſcopic ſtars, and then followed the ſtar *d* 35″ *R. Aſcenſ.* And at 10h. 30′ the fix'd ſtar was diſtant from *Jupiter*'s neareſt limb 4′ 18″.

On the 8th of *June* 10h. 20′ *Jupiter* followed the other ſtar *e* 1′ 30″ *R. Aſcenſ.* and the diſtance of *Jupiter*'s neareſt limb from the ſtar was immediately taken by the micrometer 7′ 30″.

On the 5th of *July* 8h. 26′ app. time, *Jupiter* and *Venus* were in cloſe conjunction; and ſhe being then more northerly preceded *Jupiter* in *R. Aſcenſ.* 1′ 20″. The mean diſtance of their centers from 10 ſeveral obſervations was 13′ 36″. Theſe three obſervations, made at *London*, were communicated by Mr. *Folkes*.

On the 3. of *Auguſt* 12h. 20′ eq. time, *Mars* being almoſt acronic followed the ſtar which preceeds *Bayer*'s τ in *Aquarius* 10′ 58″ of time, or 2° 44′ 57″ *R. Aſcenſ.* and he was but 36″ more ſoutherly than the ſtar: Whence having the place of the ſtar in the *Catal. Britan.* the place of *Mars* will be 7° 18′ 10″ of ♓ with 6° 38′ 10″ S. Lat.

On the 10th of *Auguſt* 11h. 50′ eq. time *Mars* follow'd the leſſer ſtar, which preceeds τ in *Aquarius* 1° 39′ 30″ *R. Aſcenſ.* but was more ſoutherly than the ſtar 10′ 42″.

On the 16th of *Auguſt* 7h. 18′ eq. time *Spica Virginis* preceded *Venus*'s center 5″ and ¼ of time, or 1′ 20″ *R. Aſcenſ.* and was more ſoutherly than the planet 18″ and ¼ of time or 4′ 35″.

On the 17th of *Auguſt Mars*, being acronic the day before and near the earth, was obſerv'd near two contiguous ſmall ſtars, the northern of which was in 3° 5′ 50″ of ♓ with 6° 6 and ¼ S. Lat. but the other being more ſoutherly was in

3° 5′ 30″ of ♓, with 6° 10′ and ¼ S. Lat. nearly: But at 10h. 40′ eq. time *Mars* followed the ſoutherly ſtar 41′ 40″ *R. Aſcenſ.* and was ſtill more ſoutherly than the ſtar 7′ 50″.

On the 18th of *September* 9h. 20′ eq. time *Mars* was obſerv'd to preceed the 53d ſtar of *Aquarius* in *Catal. Britan.* 3′ 45″ of time or 56′ 24″ *R. Aſcenſ.* and at the ſame time the ſtar was but one diameter of *Mars* more northerly than his northern limb. The place of the ſtar was in 29′ 57″ and ½ of ♒ with 4° 48′ and ½ S. Lat.

On the 30th of *October* 5h. 45′ app. time in the evening, *Mars* was near the two contiguous ſtars next *Bayer*'s *h* in *Aquarius*, and which are the 73d and 74th of *Aquarius*, in *Catal. Britan.* He had paſs'd the ſtreight line drawn thro' them, and the angle form'd at *Mars*'s center was a right angle as to ſenſe: The northerly of the two ſtars had the ſame declination with the ſouthern limb of the planet. At 5h. 53′ the diſtance of the ſtar from *Mars*'s center was 2′ 30″. At 5h. 56′ *Mars*'s center was diſtant from the third and more ſoutherly ſtar at *h* or the 75th of *Aquarius* 17′ 4″. At 6h. 18′ his center was diſtant from the northern or 73d ſtar 3′ 5″: Hence we may conclude, that *Mars* was in conjunction with the northern ſtar at 5h. 30′ nearly, and had left it only 1′ to the north. The place of the ſtar by the *Catal. Britan.* was at that time in 10° 29′ of ♓ with 1° 40′ and ¼ S. Lat. The 74th ſtar was in 10° 29′ 50″ of ♓ with 1° 44′ and ¼ S. Lat.

On the 16th of *November* 19h. 18′ eq. time, *Venus* preceeded the ſouthern ſcale of *Libra* 3′ 13″ of time, or 48′ 23″ *R. Aſcenſ.* and at the ſame time the center of the planet was more northerly than the fix'd ſtar 7′ 45″. *Venus* was ſtationary, as it were, at her aſcending node.

On the 3d. of *December* 19h. eq. time *Saturn* preceeded the third ſtar near ζ of *Libra*, or the 29th ſtar of *Libra* in *Catal. Britan.* 46″ of time or 11′ 32″ *R. Aſcenſ.* But he was more ſoutherly than the fix'd ſtar 15′ 29″, the difference being taken by the micrometer: Whence *Saturn*'s place was in 20′ 25″ and ¼ of ♏, with 2° 5′ and ¼ N. Lat.

In *Phil. Tranſ.* N° 357, there is an account of a lunar eclipſe, obſerv'd at *Cambridge* in *New England* on the 15th of *March p. m.* 1717 O. S. but in *England* it could not be ſeen by reaſon of clouds; the eclipſe ended there at 11h. 42′ and ½, which was the only obſervation had of it at that time: But afterwards Admiral *Candler* brought from *America* and com-

communicated the phaſes of that eclipſe, obſerv'd at *Lima* in *Peru* by *S. Petro Peralta*, Profeſſor of Mathematics, and publiſh'd there.

The eclipſe began at *Lima* at 8h. 41′ 8″, and ended at 11h. 19′ 55″.

Admiral *Candler* himſelf obſerv'd at the iſland of *Virgine Gorda* the eclipſe end at 12h. 13′ *p. m.* the ſky being then very clear.

In the Memoirs of the Royal Academy at *Paris* for the year 1717, there are two obſervations of this eclipſe agreeing pretty well with each other, the one by M. *Caſſini* and the other by M. *De la Hire* in the Royal Obſervatory at *Paris*: The latter eſtimated the beginning at 13h. 54′; but the end with greater certainty at 16h. 38′ 10″: The former eſtimated the beginning at 13h. 55′, and the end at 16h. 38′ 25″: The greateſt obſcuration according to M. *De la Hire* was ſeven digits and $\frac{1}{3}$; and according to M. *Caſſini* ſeven digits and $\frac{1}{2}$.

Hence, the end having, as it ſeems, been obſerv'd at each of the ſaid places with greater accuracy, the differences of Long. of *Paris* and *Lima* will be 5h. 18′ 20″; between *Paris* and *Cambridge* in *New England* 4h. 55′ 50″; between *Paris* and the iſland of *Virgine Gorda* 4h. 25′ 20″: From which if you ſubſtract 9′ 40″, the Long. of *Lima* to the weſt of *London* will be 77° 10′; of *Cambridge* in *New England* 71° and $\frac{1}{2}$, and laſtly, of *Virgine Gorda* 63° 55′: Whence the geographical ſituation of the neighbouring iſlands may be corrected with certainty.

On the 9th of *September* 1717 in the evening another eclipſe of the moon was obſerv'd at *Paris* by the ſame gentlemen and M. *Maraldi*; at *London*, the end was obſerv'd at the Royal Society's houſe at 7h. 26′; at *Paris* M. *Caſſini* obſerv'd it at 7h. 34′ 50″; M. *Maraldi* at 7h. 35′ 30″; and M. *De la Hire* at 7h. 34′ 15″; and at the ſame time at *Norimberg* M. *Wurtzelbaur* obſerv'd it at 7h. 10′ 45″: Hence, eſpecially by *Maraldi*'s obſervation, is confirm'd the difference of meridians between *London* and *Paris*, *viz.* 9′ 30″; as alſo between *London* and *Norimberg* 44′ 45″, as we have often found it before: Moreover, on the fifth day after the eclipſe, to wit, the 14th of *September* in the evening, at *Paris* the moon hid *Palilicium*, being accurately obſerv'd there by M. *Maraldi* and M. *De Liſle* the younger; at 9h. 11′ 35″ the ſtar diſappear'd over againſt the *Macula Grimaldi* or *Palus Mareotis*; but at 10h. 3′ 55″ it emerged out of the obſcure limb of the moon:

moon: As to the obſervation of this occultation at *London* vide *Phil. Tranſ.* N° 357.

Theſe obſervations, in which equated time is applied, were communicated by Mr. *Pound*; and being taken with a 15 foot teleſcope, may be depended upon as very accurate.

Some Remarks on an Eſſay of M. Caſſini's, *wherein he propoſes to find by Obſervation the* Parallax *and Magnitude of* Sirius; *by Dr.* Halley. Phil. Tranſ. N° 364. p. 1.

IN the Memoirs of the Royal Academy of *Paris* for the year 1717, there is a very remarkable eſſay by M. *Caſſini*, concerning the annual parallax of the fix'd ſtars, and particularly of *Sirius*; and in concluſion, he determines the diameter of *Sirius* to be as much bigger than that of the ſun, as the diameter of the ſun is greater than that of the earth, which he ſuppoſes to be 100 times: Now the diſtance from the ſun to the earth being certainly about 100 diameters of the ſun, it will follow, that the globe of *Sirius* muſt be a ſphere, whoſe diameter muſt equal the diſtance between the earth and ſun.

To prove this, he tells us, that he made uſe of an excellent teleſcope of 34 *French* feet or 36 *Engliſh*, with an aperture of but one inch and a half, to take off the ſpurious rays of the ſtar, which then appeared round and ſufficiently well defin'd; and comparing his body to that of *Jupiter* (which he ſays was then 50″ diameter) he found the diameter of *Jupiter* to be 10 times greater than that of the ſtar, which, conſequently, was ſeen under an angle of about 5″; which is M. *Caſſini*'s firſt poſition.

Then he tells us, that to make the obſervations of the parallax of this ſtar with all the exactneſs poſſible, he made uſe of a teleſcope of three foot in a copper tube, having fixed, in the common *focus* of the two glaſſes, four threads croſſing each other in the center under angles of 45 degrees: This tube he firmly fix'd to the plane of a mural arch, which had been upwards of 30 years immoveably cemented to the wall of the Royal Obſervatory; to which he choſe to fix it, becauſe of its ſolidity, and its being, therefore, the leſs liable to ſhake; for, having ſtood 30 years, there was no fear of its ſettling any farther in the ſpace of one year; beſides, it might eaſily be perceived if any ſuch alteration ſhould happen to it.

M. *Caſſini* having, therefore, fix'd his three foot tube as above; ſo that about the beginning of *April* 1714, N. S. (Dr. *Halley* ſuppoſes, becauſe *Sirius* was at that time in ſquare

to

to the ſun) the ſtar being exactly in the meridian, paſs'd over the center of the tube, he obſerv'd, that on the 20th of *April* the ſtar touch'd the horizontal thread with its under edge, being apparently all above it in the inverting tube, but really below it. On the 15th of *May* and 6th of *June* it paſs'd again by the center. On *June* 27. it appear'd a little under; and on *July* 9. it was found to touch the under part of the thread: On *October* 5th it again paſs'd by the center; but on *December* 29th it touch'd the upper part of the thread: *January* 18th 1715, the coldeſt day of that winter, it paſs'd exactly by the center; and on the 27th of *March* and the 1ſt of *April* it almoſt touch'd the upper ſide of the horizontal thread, from which it ſeemed a little ſeparated: But on *June* 7th it paſs'd a little under the center; and on *June* 29th the ſun being then in conjunction with *Sirius*, it paſs'd under the thread, ſo as to touch it with its upper edge: Whence it appears, that in the ſpace of a whole year, there had been no other variation of the meridian altitude of *Sirius* than the breadth of the thread, which appeared equal to the diameter of the ſtar, which M. *Caſſini* takes to be five, or at moſt ſix ſeconds.

Suppoſing this to be ſo, M. *Caſſini* then ſhews, that the whole diameter of the annual orb is to the diſtance of *Sirius*, as the ſine of 6″ to the ſine of 39° 33′ the Lat. of the ſtar: Whence the aforeſaid immenſe magnitude of its body is a neceſſary conſequence.

But before this be entirely admitted; it may not, perhaps, be amiſs to enquire, whether the ſuppos'd viſible diameter of *Sirius* were not an optic fallacy, occaſioned by the great contraction of the aperture of the object glaſs: For, we all know, that the diameters of *Aldebaran* and *Spica Virginis* are ſo ſmall, that when they happen to immerge on the dark limb of the moon, they are ſo far from loſing their light gradually, as they muſt do, were they of any ſenſible magnitude, that they vaniſh at once with their utmoſt luſtre, and likewiſe emerge in a moment, not ſmall at firſt; but at once appear with their full light, even tho' the emerſion happen very near the cuſp; whence if they were four ſeconds in diameter, they would be many ſeconds of time in getting entirely clear of the limb: But the contrary appears to all thoſe that have obſerv'd the occultations of theſe bright ſtars; and tho' *Sirius* be bigger than either of them, yet he is by far leſs than two of them; and conſequently, his diameter to theirs is leſs than the ſquare root of 2 to 1, or than 14 to 10: whence, in M. *Caſſini*'s excel-

excellent 36 foot teleſcope, thoſe ſtars ought to be about 4″ in diameter; and they would undoubtedly appear ſo, if view'd after the ſame manner; whereas, we are otherways aſſur'd, that they are leſs than a ſingle ſecond in diameter; the great ſtrength of their native light forming the reſemblance of a body, when it is nothing elſe but the ſpiſſitude of their rays.

As to the other part of the argument, to wit, that the alteration of the declination of *Sirius*, on account of the acceſs of the earth in *December* and its receſs in *June*, amounts to 6″; Dr. *Halley* only remarks, that, beſides that a radius of three foot, ſuch as M. *Caſſini*'s ſeems to have been, is ſomewhat too ſmall for ſo extremely nice an obſervation; 6″ being ſubtended by the $\frac{1}{1000}$ part of an inch; ſome of the above-mentioned obſervations plainly ſhew, that the refraction of the medium did intermix with thoſe differences, that might be occaſioned by the parallax.

But the principal objection againſt the concluſion of this argument, ſeems to be, that the meridian altitude of *Sirius* at *Paris* being under 25 degrees, the ordinary refraction of the ſtar is 1′ 55″ or 115 ſeconds; and the barometer riſing and falling upwards of two inches in 30, ſhews, that the denſity of the air, on that account, may be $\frac{1}{15}$ part more at one time than another: Whence the refractions being always proportional to the denſity of the medium, as it has been often demonſtrated by Mr. *Hauksbee*, both *in vacuo*, and in a twice or thrice condens'd air; it is plain, that in that altitude, the refraction of a ſtar may differ about ſeven or eight ſeconds, or $\frac{1}{15}$ part of 115″, which is more than the whole parallax, ſuppos'd to have been obſerv'd.

It were to be wiſh'd, that M. *Caſſini* would try this matter by the *Lucida Lyræ*, inſtead of *Sirius*, which tho' ſomewhat leſs than him, is as near to the ſolſtitial colure, and has much greater Lat. being but 28 degrees from the pole of the ecliptic, whence its parallax would be ſo much greater; and at *Paris* being within 10 degrees of the zenith, the grand objection of the difference of refraction would be almoſt entirely remov'd.

An Account of the external maxillary and other ſalival Glands; *as alſo of the inſertions of all the* Lymphatics (*both above and below the ſubclavians*) *into the* Veins; *by Dr.* Rich. Hale. Phil. Tranſ. N° 364. p. 5.

THE external maxillary glands in brutes are of the conglomerate kind; they lie externally, laterally, lengthways, on the lower jaw, partly under the *depreſſor labiorum*, and partly under the *buccinator*: A ſtrong membrane intervenes between theſe glands and the jaw on one ſide, and between them and the buccal glands on the other ſide: They are more or leſs red, like the *pancreas*, according to the quantity of blood remaining in them; otherways their ſubſtance is white.

Theſe glands receive arteries from the external carotids; veins from the external jugulars, and nerves from the third branch of the fifth pair.

The number of excretory ducts from theſe glands is not always the ſame in the ſame ſpecies of animals: In cows generally 14 are diſcovered by the probe; their orifices are valvular, about four times leſs than their ducts; every duct is about $\frac{1}{2}$ an inch from the next; thoſe in the middle of the glands are largeſt, becauſe there the glands are broadeſt and thickeſt: The ducts neither communicate with each other or with the buccal; every duct is formed of leſſer ducts united together, which riſe from the lobules (throughout the whole ſubſtance of the glands) which conſtitute each diſtinct lobe, and has the ſame ſtructure with the pancreatic duct; each lobe is depreſſed on its ſides, where it is joined to other lobes; and between the lobes ſeveral buccal glands are interſperſt.

In calves ſeldom more than 6 or 7 ducts admit any probe; when the animal grows older, the ducts appear more plain and open.

In ſheep ſix excretory ducts are always found in each external maxillary gland.

In dogs and cats, *&c.* theſe ducts are fewer, in proportion to the ſmallneſs of the glands: It is obſervable, that theſe ducts in dogs open obliquely towards the mouth, whereby the *ſaliva* may be the better mixed with the food in maſtication; which from any other ſtructure of ducts in theſe animals, that ſwallow greedily might be ſwallowed unmixt.

Dr. *Wharton cap.* 21. is the firſt who mentions the external maxillary glands; what he ſays of them is only applicable to their appearance in human ſubjects, where they are of the conglomerate

glomerate kind and very ſmall, unleſs in ſcrophulous and venereal caſes: It is plain, that he had not ſeen them in brutes; for, in his figures (which were drawn from brutes) no notice is taken of theſe glands: He deſcribes them as very ſmall, and calls them emunctories of the nerves; which was the notion in his time concerning the uſe of the conglobate glands; and the *ſaliva* was ſaid, as in *cap.* 21. *p.* 134. *è nervoſo genere profundi.*

Steno in *Obſ. Anatom. p.* 14. juſtly cenſures *Blaſius* for aſcribing to the external maxillary glands an excretory duct opening into the mouth, like the common duct from the parotid gland: Yet *Steno*, otherways very accurate, does not truly deſcribe theſe glands, nor diſtinguiſh them from the buccal, tho' they are as diſtinct from theſe, as the ſublingual are from the external maxillary glands: *Steno* divides his buccal glands into three parts: The large ducts riſe in a line from the external maxillary glands; and how diſtinct theſe glands are from the buccal appears plainly in Fig. 4. Plate II. *&c.* *Steno*'s ſecond part of the buccal glands p. 18. *intra quæ & in mediâ parte*, are mark'd *e e* in Fig. I. *quæ alias*, &c. higher are the ſame *e e* among the *papillæ*; the third part, *quæ a ſuperiore deſcendunt*, are *a b c d.*

The external maxillary glands differ from the buccal, in bigneſs, figure, ſtructure, particular number of ducts, colour, *&c.* The buccal, labial, internal maxillary and ſublingual glands are of a yellow colour; beſides, the buccal are ſeparated from the external maxillary by a ſtrong membrane: It is true, that ſeveral of the excretory ducts of the buccal glands open near the ducts of the maxillary (whence *Steno* confounded theſe glands) but they likewiſe do ſo round his own ducts from the parotids; and ſome ducts from like glands open near the ſublingual, as alſo about *Nuck*'s ducts; in which places the buccal ducts are moſt numerous.

In ſhort, there is a very great number of excretory ducts diſperſed all over the membrane, which inveſts the mouth, *fauces*, *&c.* which riſe from glands that lie under this internal membrane: Theſe glands are more numerous in ſome parts than others, and receive different names according to the part they belong to, as labial, buccal, palatine, *&c.* But theſe are ſmall glands with one excretory duct, and tho' they ſeparate *ſaliva* like the large conglomerate glands, parotids and maxillaries, *&c.* yet they differ from theſe in ſtructure, one common excretory duct, *&c.* whereas the external maxillaries differ from all the other glands of the mouth, *viz.* from the buccal in ſeveral reſpects, beſides their colour; in which particular they are alſo diſtin-

distinguished from the internal maxillary and sublingual glands; they likewise differ from these as well as from the parotids, in having a great number of common excretory ducts: This number of excretory ducts was not observed by *Steno*; nor did he know, that these ducts in the same line were the excretory ducts of large conglomerate glands (like the parotids) distinct from the buccal.

Bartholin p. 542. mentions the external maxillary glands, but does not describe them: *Nuck Adenol.* p. 5. N° 11. only gives them a place in his catalogue of glands; but takes no farther notice of them, tho' he writ a book (*Sialog. p.* 15. 158.) chiefly about a new salival duct rising from a gland, that is found in no other animal besides a dog.

Mr. *Cowper* had never observed these external maxillary glands, as appears by a letter of his to Dr. *Hale* in answer to one the Dr. sent him upon the first discovery of these glands: The external maxillary glands of the conglobate kind in men are marked *g* in Fig. 1. of his *Myotomia reformata.*

The ducts of the external maxillary glands are opposite to the orifices of *Steno*'s ducts, from which glands and ducts, as also from the buccal, labial and gingival glands, the *saliva* flows from all parts of the mouth without the teeth; from *Wharton*'s and the sublingual ducts, from the tonsils, *fauces*, *fretum Stenonis*, gingival, lingual and palatine glands, the *saliva* is derived from the upper and lower, fore and hinder-parts of the mouth within the teeth.

What has been said of these salivary glands, *&c.* will be best understood by the following figures, drawn in *October* 1697 at *Trin. Coll. Oxon.* by Mr. *Burghers*, and since compared with the parts themselves in cows, calves, *&c.* these figures are part of many more taken from preparations at the same time.

The insertions of all the lymphatic vessels into the veins can be discovered but in few subjects, and no figure has hitherto been given of them.

These figures shew the course of the *lympha* both below and above the subclavian veins in men and axillary veins in dogs. The *lympha* below the *receptaculum chyli* is conveyed from all the inferior parts by a great number of small lymphatic vessels, which uniting with others obliquely above the valves, become proportionably bigger, till at length they constitute two large trunks near the emulgents, which are the *pedunculi* or beginnings of the *receptaculum chyli*: The *lympha* from the parts above the subclavian veins is derived in like manner from lesser lymphatics to the common ducts here delineated.

Pecquet has given a figure of the thoracic duct in a dog, which is double from the receptacle, and inserted by four branches into each axillary vein: Dr. *Hale* supposes with *Bartholin* p. 616, 620. (who has borrowed this figure from *Pecquet*) that such an insertion is a *lusus naturæ*: For, tho' the thoracic duct may be double, and is sometimes divided into two parts near the subclavian veins; yet generally, it is single, the *lympha* from all parts on both sides of the body being conveyed by proper *lymphæducts* into one common thoracic duct, which conveys this liquor together with the chyle from the lacteals into the left subclavian vein by one, three or more branches. For, there is as great a variety in the number of these branches as in the places of their insertion.

Mr. *Cowper* injected the thoracic duct in a human subject and gave a figure of that preparation in his book of anatomy: But this figure is imperfect, and the insertion of the thoracic duct so ill drawn, that little can be learnt by it: However, no anatomist has given any figure, that shews the insertions of the lymphatics from both arms and both sides of the head, *&c.* above the subclavian veins, which appear so plain in the following figures.

Fig. 1. Plate I. represents the passages and vessels, by which the chyle and *lympha* pass into the veins of a dog; 12, 12 the lymphatics that convey *lympha* from the thighs and lower parts; 13, 13 lateral lymphatics, arising from the groin, testicles and neighbouring parts; 14 the receptacle of the chyle; 15 an indenture in the receptacle, thro' which one tendon of the diaphragm passes; 16 lymphatics from a neighbouring gland; 17 some lymphatics from the diaphragm; 18 an artery that supplies the loins and runs thro' a division of the receptacle; 19 the *pancreas Asellii*; 20 the *vasa lactea secundi generis*; 21 the beginning of the thoracic duct; 22 some divarications of the duct; 23 the continuation of the duct and its progress; 24 the descending *aorta*.

N. B. That the arteries at 18 and 24 (as also the tendon at 15) by their pulsation do much promote the ascent of the chyle and *lympha*.

Fig. 2. At 25 represents a common divarication of the duct; 26 a lymphatic from some neighbouring gland; 27 a double lymphatic from the secondary gland 42, as in Fig. 3. 28 that part of the thoracic duct, where both its branches, and the lymphatics from the left side of the head and left fore-leg meet; 29 the lymphatics from the left side of the head and left fore-leg united; they lie on the inside of the vein; 30 a lymphatic with

a pin

Fig. II.

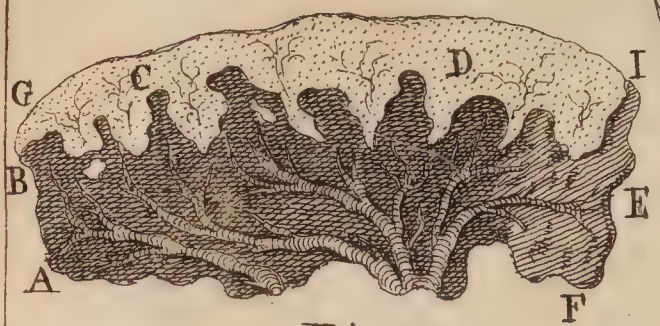


Fig. III.

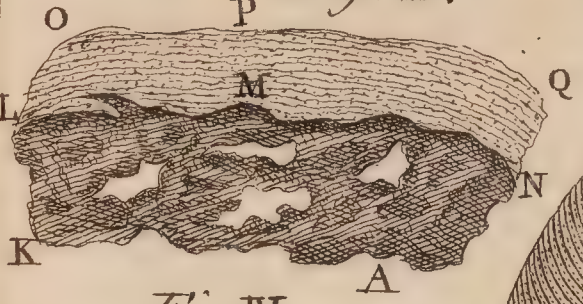


Fig. IV.

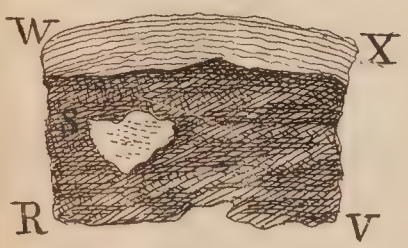


Fig. I.

Fig. VI.

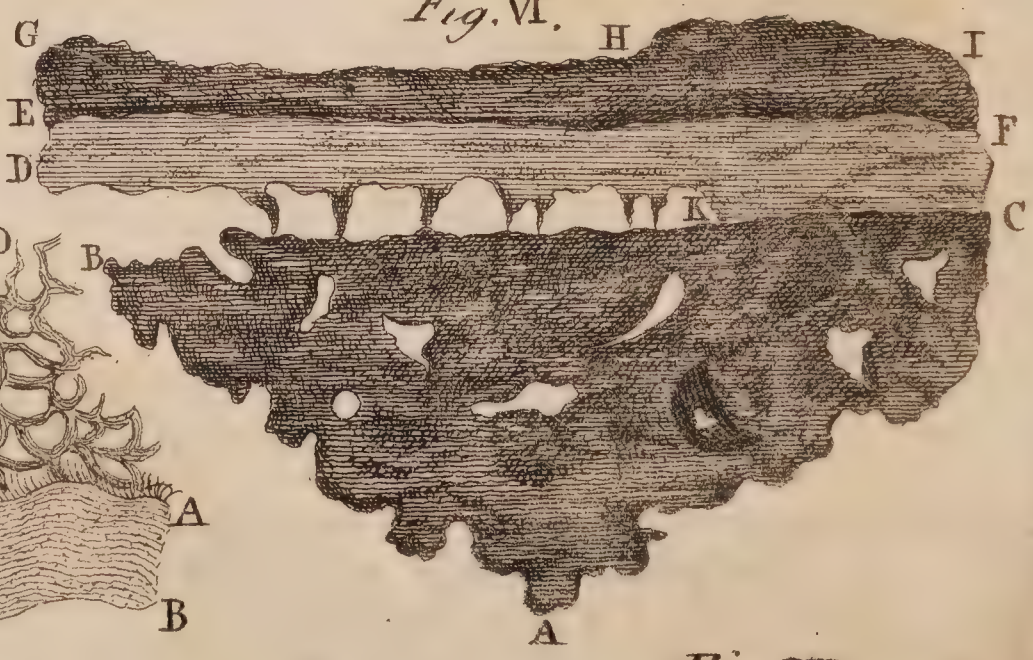


Fig. V.

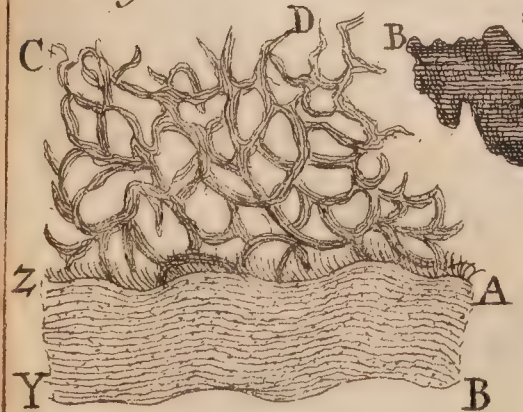
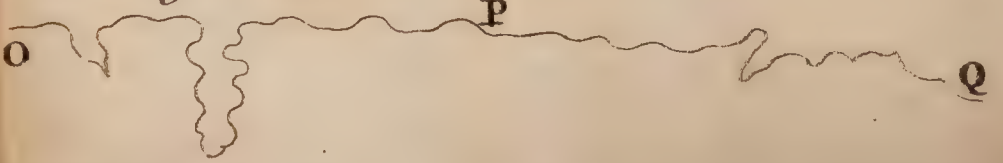
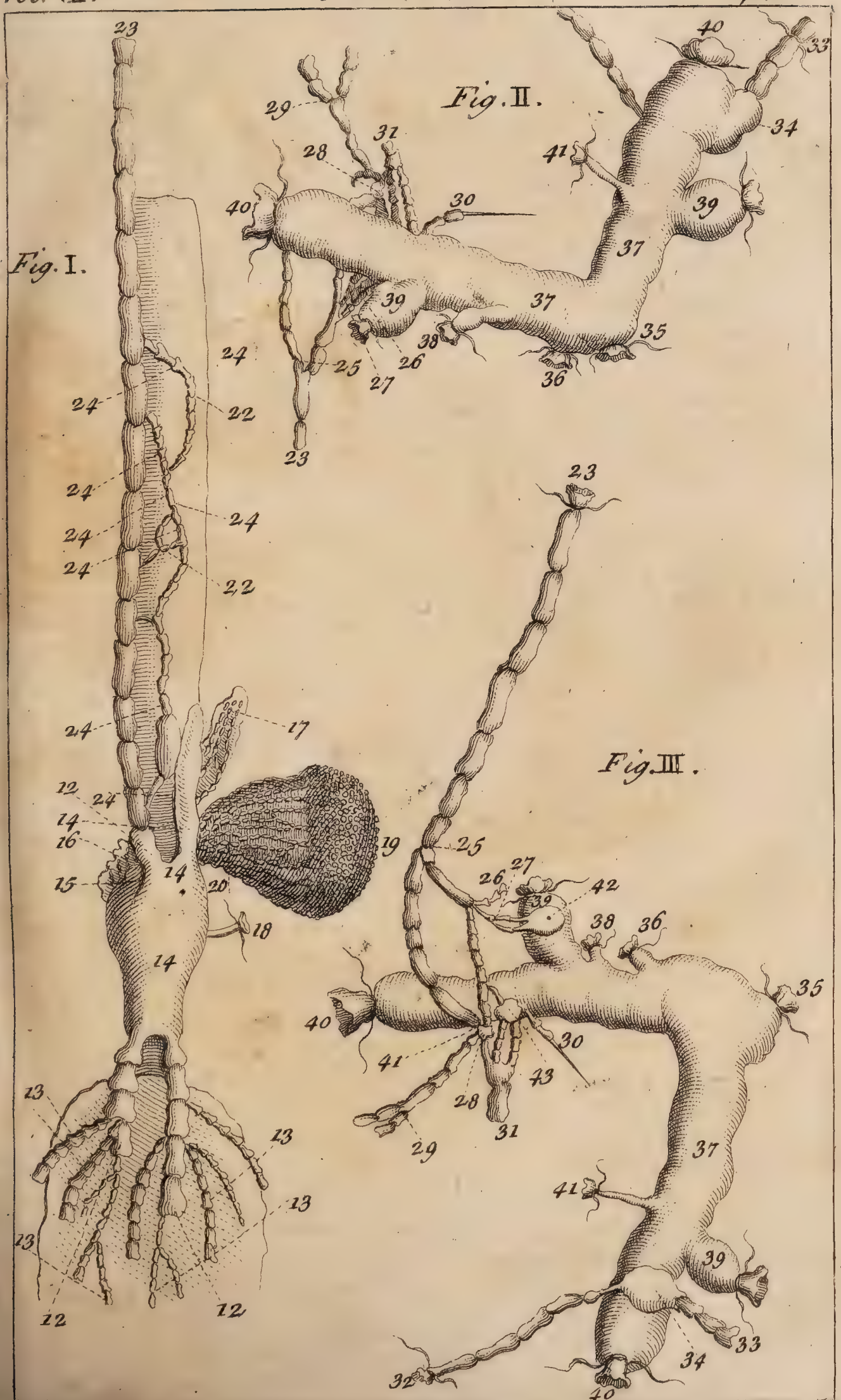


Fig. VII.

Fig. VIII.



Vol. VII. PLATE. I. p.



G. Smith del. Hulett Scul.

PLATE. II.

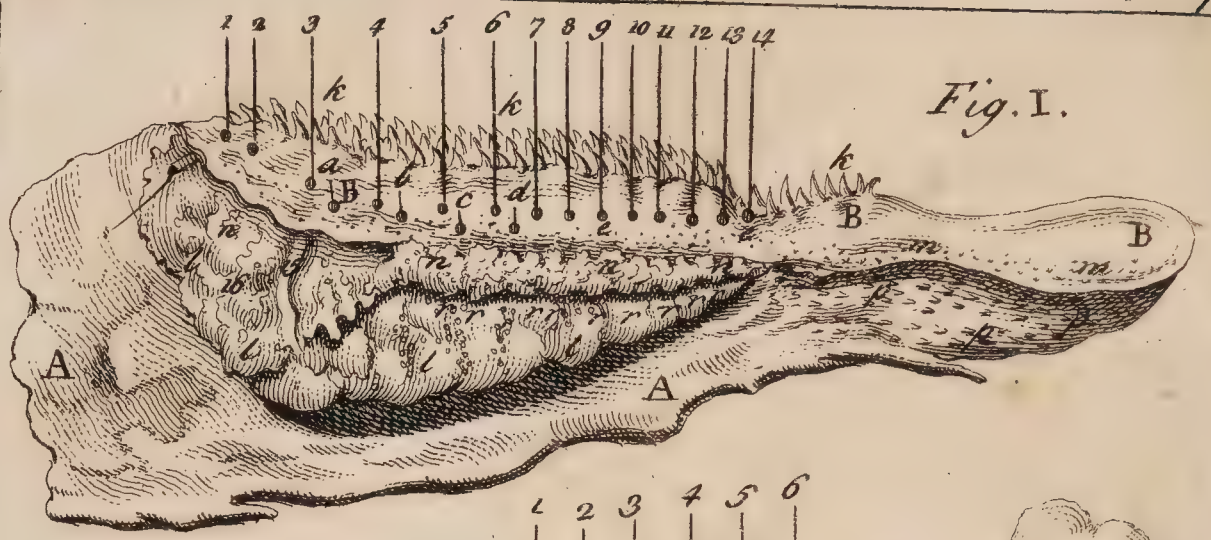


Fig. I.

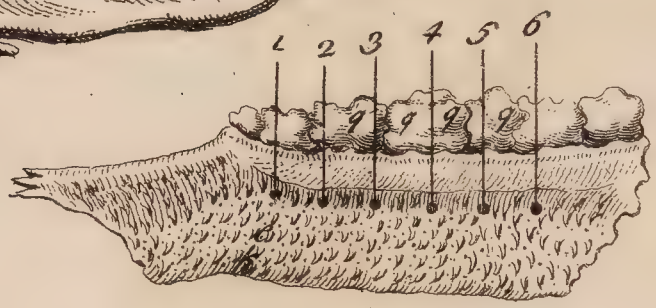


Fig. II.

Fig. III.

Fig. IV.

Fig. V.

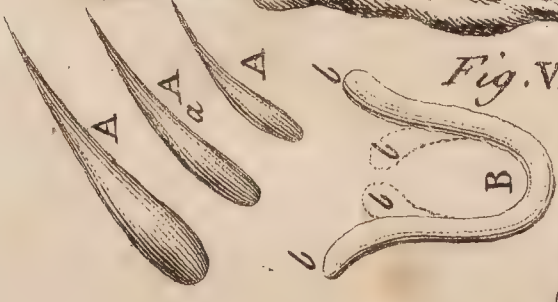
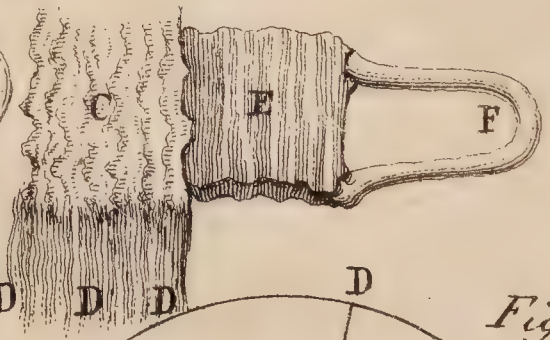


Fig. VI.



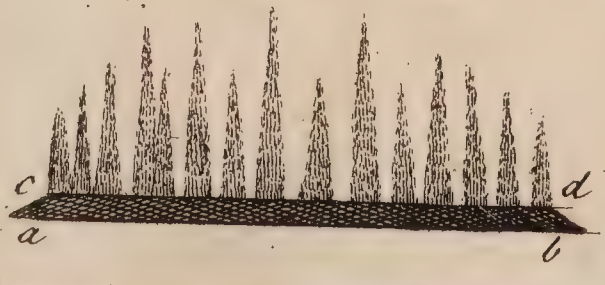


Fig. VII.

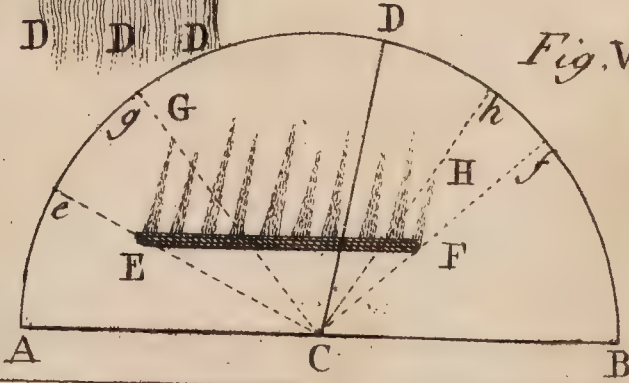


Fig. VIII.

a pin in it from a neighbouring gland, perhaps the *thymus*; 31 a lymphatic from the neck, &c. it is divided and enters the jugular by two diſtinct branches under the *ſacculus* 43; 32 the lymphatic from the right ſide of the head; 33 the lymphatic from the right fore-leg; 34 the large *ſacculus*, or receptacle of the *lympha*, on the right ſide, that receives all the *lympha* on that ſide and conveys it into the jugular vein; 35 the deſcending *cava*; 36 the mammary vein, which is ſometimes ſingle; 37 the ſubclavian veins; 38 the vertebral vein, 39 the axillary veins; 40 the jugular veins; 41 the right internal jugular not injected; 42 a ſmall ſecondary lymphatic gland on the back part of the top of the *thorax*, 43 the *ſacculus*, that receives all the chyle and *lymyha* from the whole body (excepting 30, 31, 32, 33, 34) and diſcharges it into the vein; at leaſt we know of no other lymphatics that any where elſe enter the veins; 44 a lymyhatic (or membrane, for it was not injected) that joins 29 to the largeſt branch of the thoracic duct.

Fig. 3. repreſents the upper part of Fig. 2. reverſed; the duct, &c. being turned up, that the inſertion, both *ſacculi*, &c. may be the better diſcovered; this figure is to be explained by the preceeding; and has only from 42 to 44 more than the upper part of Fig. 1.

N. B. In this ſubject the chyle and *lympha* are emptied into the jugular and not into the axillary veins; they are ſometimes emptied partly into the jugular, and partly into the axillary or ſubclavian vein; in men, generally into the ſubclavian.

Fig. 1. Plate II. repreſents part of the left cheek of an ox ſeparated from the lower jaw-bone, with the external maxillary glands, its ducts, &c. 1, 2, 3, &c. to 14 are briſtles inſerted into the ducts of the external maxillary gland *lll*; theſe ducts open ſloping into the mouth, for the better mixture of the *ſaliva* with the food; 15 the duct 3 injected with wax to diſcover its diviſion and bigneſs in reſpect of the orifice; 16 a lobule of the maxillary gland; its excretory duct is injected with wax and ends at 15; 17 the duct 1 laid bare and opened to ſhew its large cavity, &c. A A part of the muſcles and fat, &c. belonging to the lower jaw; B B part of the internal membrane that inveſts the mouth; *abcd* briſtles in thoſe ducts of the buccal glands *nn*, that would admit any; *eee* thoſe orifices of the buccal glands, that were too little to admit briſtles; *kkk* the *papillæ* on the inſide of the mouth; *lll* the lobes that conſtitute the external maxillary gland; *mmm* the orifices of the labial glands *pp*, that were too ſmall for paſſing briſtles; *nnn* buccal glandules interſperſed

ſperſed between the lobules of the maxillary gland; *nnn* near *rrr*, part of the buccal glands, where they appear thickeſt, being raiſed to ſhew the ducts *rrr*, running under them; *ppp* the labial glands like the buccal; Mr. *Cowper* in his Fig. 4. repreſents them at H H; *rrr* the ducts marked 6 to 14, as they appear under the glands *nn*.

N. B. The ſame numbers and letters repreſent the ſame things in the following figures.

Fig. 2. ſhews part of the left jaw-bone and cheek of a ſheep, where the briſtles 1, 2, 3, &c. repreſent the conſtant number of excretory ducts from the external maxillary gland in theſe animals.

Fig. 3. ſhews part of the right cheek of a dog taken from the lower jaw-bone: *f* the orifice of *Steno*'s ſalival duct; *g* the orifice of *Nuck*'s duct, which riſes as a *papilla* on the membrane B B; *h Nuck*'s new duct, not found in men, oxen or ſheep, but in dogs, their orbit not being entirely bony; *i Nuck*'s gland; *ooo* the orifices of ſome excretory ducts, belonging to the external maxillary gland, that were too ſtrait for the admiſſion of briſtles; *q q* the teeth; in this ſubject they are the teeth of the upper jaw, near the ſecond of which the orifice of *Nuck*'s duct appears.

Fig. 4. ſhews the back part (next the *cutis*) of the external maxillary gland of the ſame dog, as it is inveſted with the buccal glands.

Fig. 5. repreſents the external maxillary gland in the right cheek of a cat: In this ſubject the Dr. could only probe two ducts; three &c. would not admit briſtles.

An Account of the Plague *at* Conſtantinople; *by Dr.* Timoni. Phil. Tranſ. N° 364. p. 14. *Tranſlated from the* Latin.

IT is manifeſt both from hiſtory and experience, that the plague is communicated from *Egypt* to *Conſtantinople*; and tho' ſome ſeeds of the old infection ſtill remain; yet new matter is at times conveyed: It is pretty well ſuppreſſed by the intenſe cold of the winter; yet here and there ſome ſigns of it appear, both in winter and ſpring; it gathers ſtrength in ſummer, and is at its greateſt height in autumn: Tho' the northerly winds (that blow in ſummer at ſtated times) be here ſomewhat cold; yet they do not ſtop the ſpreading of the peſtilential venom: If the ſoutherly winds, that are pretty warm, do conſtantly blow, they ſuppreſs the plague in ſummer: The ſymptoms

toms of the plague at *Conſtantinople* are exactly the ſame with thoſe of the plague at *Numegen* in 1636, 1637, as deſcribed by *Diemerbroeck*; it ſeiſed on man *cap.* 4. and one or two horſes, dogs or cats were infected with peſtilential *bubo*'s, of which they died. The common people (eſpecially the poorer ſort) among *Turks*, *Chriſtians* and *Jews* are perſuaded that the plague is a judgment from heaven; and therefore, they do not guard againſt the contagion; but the better ſort, eſpecially the *Chriſtians*, take all poſſible precaution.

The ſymptoms according to *Diemerbroeck cap.* 7. are, fevers, *bubo*'s, carbuncles, *exanthemata*, head-ach, phrenſy, ſleepineſs, watchings, anxiety, great weakneſs, diſordered viſion, palpitation of the heart, drineſs of the tongue, vomiting, ſighing, worms, looſeneſs, hemorrhage at the noſe, flowing of the *menſes*, voiding of blood by the *urethra*, ſpitting of blood, pains of the ſide, liver, reins and other parts.

To theſe Dr. *Timoni* adds an ulcerous laſſitude of the joints, ſhuddering, with ſometimes a heat enſuing, and oftener none at all, a *nauſea* without vomiting, *vertigo*'s and vertiginous motions, and a trembling of the hands on the firſt onſet: None of all the above-mentioned ſymptoms is inſeparable from the plague; nay, theſe three, to wit, *bubo*'s, carbuncles and *exanthemata* are not peculiar thereto, and often there is no fever: So that the following general rule may be laid down; *viz.* 'when there are plain ſigns of the plague, we may cer-'tainly conclude, the diſtemper to be the plague; but when 'there are no ſuch ſigns, we cannot certainly conclude the con-'trary:' For, after ſeveral have been ſeiſed with the plague, and a gentle *rigor* has preceeded, none of the above-mention'd ſigns appear for ſeveral days together, but afterwards they break out plentifully: Some, after catching the infection, are only ſeiſed with a very ſlight languor, and ſoon after they go about their ordinary buſineſs without any inconveniency; but on the third or fourth day they fall down ſuddenly, and expire on the high-way, and after the patient's death black ſpots appear, which remove the ſuſpicion of an apoplexy or any other diſorder; and this is likewiſe confirmed both by the antecedent manifeſt cauſe of the infection, and by the contagion afterwards ſpreading: Several walk about without any manifeſt ailment and are cured, even when the *bubo*'s and carbuncles break out; yet the patients generally fever, and are very bad: Vomiting, looſeneſs, together with a ſudden weakneſs, and a fever, are ſigns of the plague, and more ſo without

a fever

a fever; but if pain be felt in the common emunctories, the plague is not to be concluded from ſuſpected ſigns.

A reddiſh puſtule, half as big as a vetch, and ſuppurating, is malignant; for, it ſoon becomes a livid carbuncle: Carbuncles break out indifferently on all parts of the body, even on the lips, tongue, balls of the eye, *glans* of the *penis*, &c. and *bubo*'s on the emunctories only: Hard ſmall glandules about the neck are malignant: *Exanthemata* are always mortal: If *bubo*'s do ſoon ſuppurate, there is ſome hopes of the patient's recovery; and there is no hazard, if ſuch as are not ripe, be opened with a lancet: Several that have been cured by diſcuſſing the *bubo*, have afterwards, upon going to infected places, felt a kind of dull pain in that part: The plague in ſome patients may lie concealed for ſeveral days, and afterwards actually break out: If a patient, that begins to recover, be guilty of any great irregularity in diet before the 40th day, a new *bubo* breaks out, and he dies: It is reckoned a very uncommon thing, if a patient, who has been perfectly cured of the plague, ſhould be ſeiſed again the ſame year: If we reduce the diſpoſition of bodies to eight degrees; the diſpoſition of ſuch, as have not been infected, is as 7; and of ſuch, as already have, as 1: A perſon living for ſome months in an infected houſe without being touched, has at length been ſeiſed with the plague. Old people, according to *Diemerbroeck cap.* 4. are infected with more difficulty, but young people more eaſily: Strangers are more expoſed to it than the natives: The *Armenians* are of all nations leaſt ſubject to be infected; they eat but little meat, and live chiefly on onions, leeks, and garlic, and drink wine plentifully: To eat pork in the time of the plague is immediate poiſon. Nothing is ſo apt to diſpoſe to the infection, as the paſſions of the mind, eſpecially exceſſive grief and ſudden fear: Venereal patients are indifferently diſpoſed to the contagion; yet their *bubo*'s ſuppurating generally degenerate into *fiſtula*'s: Houſes kept clean and neat are not ſo eaſily infected as thoſe that are not ſo: *Cachectic* and *Icteric* patients and others in chronic diſtempers, do either eſcape, or get the better of the contagion; whilſt others of a blooming complexion, and robuſt conſtitution die of it.

Fumigations of juniper-berries, pitch and ſulphur are found to be beneficial, if continued day and night, ſo as to be conveyed from the lower parts thro' the whole houſe: Perfumes made of vinegar are reckoned to be of ſome ſervice: Oil of amber

amber rubbed on the noſtrils is very good: A moderate glaſs of wine, chearfullneſs, and a regular diet are of conſiderable ſervice, as preſervatives: Blood-letting eſpecially if late, is not adviſeable; for, oftentimes it cannot be ſtopped; and in ſcarifications the patient has been obſerved to bleed to death: Gentle vomits are of no ſervice, and ſtrong vomits are very pernicious; purges are preſent death: The only hope of relief is in ſweating and alexipharmics: At *Conſtantinople* they commonly make uſe of venice-treacle: *Bezoar*, tho' genuine, is almoſt of no manner of ſervice: The *Jews* make uſe of acids: The *Armenians* and *Greeks* of wine and ſpirit of wine: The *Chriſtians* abſtain very religiouſly for ſeveral days from meat and the broth made of it; but the *Turks* are not ſo ſcrupulous.

A ſmall tubercle breaking out without any livid ſpot on ſeveral, who have abſtained from fleſh, ſuch have been abroad and in health for a week, but afterwards eating meat they have died in a very ſhort time: Crude *opium* and all opiates are good as preſervatives, and often as curatives: About the quantity of two drachms of oil of *naptha* or white *petroleum*, drank in any vehicle at pleaſure, is reckoned a ſovereign remedy by the *Turkiſh* nobility; this agrees with camphire ſo much celebrated among us: Several patients were ſaid to be cured by drawing blood on the firſt day of the infection, till they ſwooned away, and afterwards adminiſtring a draught of ſtrong vinegar, in which a drachm of *Sang. Drac.* and *Bol. Armen.* was diſſolved: As to blood-letting the Dr. leaves others to determine; only he affirms, that it is a very excellent remedy: A ſtrong friction continued for ſome hours with bruiſed garlic, on the breaſt and back, oppoſite to the heart, is of ſervice; as is alſo hellebore-root put tranſverſely on the emunctories into inciſions made in the fleſh: Chickens or pidgeons, either alive, or cut thro' the middle, and applied to *bubo*'s and carbuncles, and even to the region of the heart and often repeated and not kept on above half an hour, have been found of conſiderable ſervice. Oil of amber and extract of juniper have been found to ſucceed in practice: The method preſcrib'd by *Diemerbroeck* is in all reſpects beneficial in this plague, and he has writ very well on the ſubject, only he derives the cauſe of it too far back; and *Barbette* is no contemptible author.

In the latter end of *May* 1712. the plague began at *Conſtantinople*, and in the cloſe of *July* it was at its height; in one day there died upwards of 90; at which time the ſtated nor-

therly winds blew very violently; and after them there ſucceeded a pretty ſtrong ſouth wind; the firſt week after, only 40 died in a day; the ſecond week about 30; and the third under 20, which is the common rate every day at *Conſtantinople* at this ſeaſon of the year, even in a good ſtate of ſalubrity: Thus the plague was ſuppreſſed that year in autumn; tho' it is at its height at that time and begins in the middle or latter end of ſummer; it has likewiſe been obſerved, that the plague beginning in ſpring was ſuppreſs'd in autumn: It is obſervable, that tho' the plague rage exceedingly in *Egypt*, yet it ceaſes infallibly after the ſummer ſolſtice; the cauſe of which is a matter of higher inquiry: This peculiar quality in the climate of *Egypt* does, as to this particular phænomenon of the plague, extend as far as *Smyrna*, the iſland of *Chio*, and even the ſtreights of the *Helleſpont*: It is farther to be obſerved, that the ſtated northerly winds, following the courſe of the ſun, convey air, impregnated not only with excrementitious vapours, but likewiſe nitrous particles: The Dr. had an opportunity of obſerving this by a kind of garment made of goats hair: This garment ſeems to be a ſort of hygroſcope; for, whatever part of it happens to be moiſtened with the ſweat of the hands or neck, and afterwards become dry, tho' wore ſeveral months after, yet in half an hour or an hour after ſunriſe (to wit, when the ſtated annual winds begin to blow) it will again contract ſuch a dampneſs, as to be ſtained with a very deep black ſpot; and in two or three hours after, the garment dries again, and the ſpot vaniſhes. In this there are two things obſervable; 1. That if the garment be ſeveral times moiſtened in fair water and afterwards dried, this phænomenon of the dampneſs and ſpot does not appear, as when it is moiſtened with ſweat. 2. That if it happen to rain at that time, the garment is not ſo eaſily moiſtened and ſtained, provided it be not actually expoſed in the rain: From which the Dr. gathers, that the air conveyed thither from the *Euxine* ſea, on the firſt blowing of the ſtated winds, is impregnated not only with watery particles, but likewiſe with nitrous ſalts; which laſt are fixed by the congener ſaline particles communicated by ſweat to the garment, and by their union form that ſtain: Therefore, the air impregnated with theſe ſalts may promote the ſpreading of the peſtilential ferment, which probably is of a ſalino-corroſive nature; and it is not unlikely, that the air of *Egypt*, convey'd thither by the ſouth wind, being very much rarified and diveſted of ſuch ſalts, may reſiſt

the

the ſpreading of the plague: Add to this, that the warm ſouth wind diſpoſes our bodies to a more plentiful perſpiration.

But if it ſhould be aſked, whether the plague coagulate or greatly diſſolve the blood; the anſwer is, that it does neither primarily and immediately, but that both may happen in proceſs of time, as the active principles of the maſs of blood in different individuals happen to vary: For, the plague primarily and immediately attacks the ſpirits, afterwards the fluids, and produces ſeveral diſorders in the ſolids, according to their various ſtate: Hence according to *Diemerbroeck cap.* 7. there are various ſymptoms in the plague, and thoſe diametrically oppoſite to each other, that cannot be cured by one ſingle remedy, but require the judgment of a ſkillful phyſician, to apply what is proper: Yet generally it is to be obſerved, that the moſt generous remedies ſhould be ſpeedily taken and in large doſes; that at firſt oleoſo-balſamics and volatiles, which are beneficial to the ſpirits, ſhould be adminiſtred; that all evacuations but ſweat ſhould be avoided; and therefore, that no artificial evacuations ſhould be attempted; that the natural ones ſhould be ſtopped by proper remedies; thus a looſeneſs was happily ſuppreſſed by an aſtringent glyſter, in which there was ſome treacle; that if the ſpirits be unruly and their motion exceſſive (which is chiefly known by watchings) they ſhould be laid by opiates; but if on the contrary languid (which is chiefly known by drouſineſs) they ſhould be rouſed by volatiles and preparations of camphire; and that the conſiſtence of the blood may be preſerved entire throughout the whole diſtemper, acids and earthy aſtringents ſhould be made uſe of, to drive the poiſon outwards.

An Account of a luminous appearance in the Air *at* Dublin; *by Mr.* Percival. Phil. Tranſ. N° 364. p. 21.

January 12. 1719-20 there began an odd appearance in the ſky about 10 o'clock; but it had nothing very remarkable till about ½ an hour after 11, when Mr. *Percival* was called out to ſee it by his ſervants, who had obſerved it about ¼ a quarter of an hour and told him it looked juſt like fire: But it firſt appeared to Mr. *Percival* in long ſtreams of light, of a round body, as repreſented at A Fig. 6. Plate II. and very bright; tho' ſome were coloured, as at *A a*; they came before the wind, which was then at weſt, as near as he could conjecture, there not being a cloud in the ſky, and the moon the brighteſt he ever knew.

They had rain about five, but at ſix o'clock the night was clear; the ſtreams of light A A moved very ſlow (there being but

little wind) but as they moved they joined, and ſwelling out in the middle, formed themſelves into the figure *b b* B, continuing to advance ſlowly in that ſhape for about a minute, when the two ends *b b*, approached near each other, as repreſented by the prick'd lines; the advanced part B ran back ſuddenly and with great ſwiftneſs, and joining itſelf with the ends *b b*, form'd itſelf into the figure C, quivering in the upper part, and darting down perpendicularly in ſharp points as at D D D; and its colour changed from a bright light into the colours of the rainbow, but much fainter: It continued thus about a minute, and then the ſharp points D D D, gathering themſelves up into C, it again changed into a ſquare ſheet of light, as at E, and ſwelled out at F, as before at B; and advancing leiſurely exhibited the ſame ſcene as before, till it ſeemed to diſperſe at a great diſtance into ſmall thin light clouds; tho' it is probable, that to thoſe who ſaw it in a like ſituation, as it went along, it might make the ſame appearance as it did to Mr. *Percival*: He was very particular in obſerving it, and he delineated it very exactly, as repreſented in the Fig. having obſerved it upwards of $\frac{1}{4}$ of an hour; The beginning of it was very like the *aurora borealis*, which was very frequent this winter in *Ireland*.

Of the infinity of the Sphere of fixed Stars; *by Dr.* Halley. Phil. Tranſ. N° 364. p. 22.

THE ſyſtem of the world, as it is now underſtood, is taken to poſſeſs the whole abyſs of ſpace, and to be as ſuch actually infinite; and the appearance of the ſphere of fixed ſtars, ſtill diſcovering ſmaller and ſmaller ones, as you apply better teleſcopes, ſeems to confirm this doctrine; and indeed, were the whole ſyſtem finite, it, tho' never ſo extended, would ſtill poſſeſs no part of the *infinitum* of ſpace, which neceſſarily and evidently exiſts; whence the whole would be ſurrounded on all ſides with an infinite *inane*, and the ſuperficial ſtars would gravitate towards thoſe near the center, and with an accelerated motion run into them, and in proceſs of time coaleſce and unite with them into one; and ſuppoſing a ſufficient ſpace of time, this would be a neceſſary conſequence: But if the whole be infinite, all its parts would be nearly *in æquilibrio*, and conſequently each fixed ſtar, being drawn by contrary powers, would keep its place, or move till ſuch time, as from ſuch an *æquilibrium* it found its reſting place; on which account ſome, perhaps, may think the infinity of the ſphere of fixed ſtars no very precarious poſtulate.

But

But to this there are two objections, which are rather of a metaphyſical than phyſical nature; and firſt this ſuppoſes, as its conſequence, that the number of fixed ſtars is not only indefinite, but actually more than any finite number; which ſeems abſurd *in terminis*, all number being compoſed of units, and no two points or centers being at a diſtance more than finite: But to this it may be anſwered, that by the ſame argument we may conclude againſt the poſſibility of eternal duration, becauſe no number of days, or years, or ages, can compleat it.

Another objection urged againſt it is, that if the number of fixed ſtars were more than finite, the whole ſuperficies of their apparent ſphere would be luminous; for, that thoſe ſhining bodies would be more in number than there are ſeconds of a degree in the area of the whole ſpherical ſurface, which the Dr. thinks cannot be denied: But if we ſuppoſe all the fixed ſtars to be as far from each other, as the neareſt of them is from the ſun; that is, if we may ſuppoſe the ſun to be one of them, at a greater diſtance their diſks and light will be diminiſhed in the ratio of ſquares, and the ſpace to contain them will be increaſed in the ſame ratio; ſo that in each ſpherical ſurface the number of ſtars it might contain will be as the ſquare of their diſtances: Suppoſe the diſtances immenſely great, as we are well aſſured they muſt be; and thence by an obvious calculation it will be found, that as the light of the fixed ſtars diminiſhes, the intervals between them decreaſe in a leſs proportion; the one being as the diſtances, and the other as the ſquares thereof reciprocally: Add to this, that the more remote ſtars, and thoſe far ſhort of the remoteſt, vaniſh even in the niceſt teleſcopes, by reaſon of their exceeding minuteneſs: So that, tho' it were true, that ſome ſuch ſtars are in ſuch a place; yet their beams, aided by any help hitherto known, are not ſufficient to ſtrike our ſenſe; after the ſame manner as a ſmall teleſcopic ſtar is by no means perceivable by the naked eye.

Of the Number, Order and Light of the fixed Stars; *by the Same.* Phil. Tranſ. N° 364. p. 24.

DR. *Halley*, having in the preceeding *Tranſaction* propoſed ſome arguments, that ſeemed to him to evince the infinity of the ſphere of fixed ſtars, as poſſeſſing the whole abyſs of ſpace, or the το παν, which at preſent is generally underſtood to be neceſſarily infinite, thence laid down what may ſeem a very metaphyſical paradox, *viz.* that the number of fixed ſtars muſt conſequently be more than any finite number, and ſome of them

more

more than at a finite diſtance from others. This ſeems to involve a contradiction; but it is not the only one that occurs to thoſe who have undertaken freely to conſider the nature of infinite, which, perhaps, the very narrow limits of human capacity cannot attain to.

The Dr. having attentively examined what might be the conſequence of an hypotheſis, that the ſun being one of the fixed ſtars, all the reſt were as far diſtant from each other, as they are from us, by a due calculation finds, that there cannot, upon that ſuppoſition, be more than 13 points in the ſurface of a ſphere, as far diſtant from the center of it, as they are from each other; and he believes it would be hard to find how to place 13 globes of equal magnitude, ſo as to touch one in the center: For, the 12 angles of the *icoſaedron* are very little more diſtant from each other than from its center; that is, the ſide of the triangular baſe of that ſolid is very little more than the ſemidiameter of the circumſcribed ſphere, it being to it as 21 to 20 nearly: So that it is plain, that ſomewhat more than 12 equal ſpheres may be poſited about a middle one; but the ſpherical angles or inclinations of the planes of theſe figures being incommenſurable with the 360 degrees of the circle, there will be ſeveral interſtices left between ſome of the 12, but not ſuch as to receive in any part the thirteenth ſphere.

Hence it is no very improbable conjecture, that the number of the fixed ſtars of the firſt magnitude is ſo ſmall, becauſe this ſuperior appearance of light ariſes from their nearneſs; thoſe that are leſs, ſhewing themſelves ſo ſmall by reaſon of their greater diſtance: Now there are in all but 16 fixed ſtars, that can indiſputably be accounted of the firſt magnitude; whereof four are without the zodiac, *viz. Capella*, *Arcturus*, *Lucida Lyræ* and *Lucida Aquilæ* to the north; four in the way of the moon and planets, to wit, *Palilicium*, *Cor Leonis*, *Spica* and *Cor Scorpii*; and five to the ſouthward, that are ſeen in *England*, *viz.* the *foot* and *right ſhoulder* of *Orion*, *Sirius*, *Procyon* and *Fomahant*; and there are three more that never riſe in our horizon, viz. *Canopus*, *Acharnâr* and the *foot* of the *Centaur*. But that they exceed the number 13 may eaſily be accounted for from the different magnitudes obſervable in the ſtars themſelves; and perhaps ſome of them may be much nearer to each other than they are to us; this exceſs of number being found ſingly in the ſigns of *Gemini* and *Cancer*; and indeed within 45 degrees of longitude or $\frac{1}{8}$ of the whole, there are no leſs than 5 of theſe 16 to be ſeen: If therefore, their number be ſuppoſed 13, omitting niceties

niceties in a matter of ſuch irregularity, at twice the diſtance from the ſun there may be placed four times as many, or 52; which with the ſame allowance, would nearly repreſent the number of the ſtars, we find of the ſecond magnitude: So 9 × 13 or 117 for thoſe at three times the diſtance; and at 10 times the diſtance 100 × 13 or 1300 ſtars; which diſtance may perhaps diminiſh the light of any of the ſtars of the firſt magnitude to that of the ſixth; it being but the hundredth part of what at their preſent diſtance they appear with. But if, ſince we have room enough for it, we ſhould ſuppoſe the ſphere continued to 10 times the laſt, or 100 times the firſt diſtance, the number of ſtars would be 130,000, and they would appear but with the 10,000th part of the light of a ſtar of the firſt magnitude, as we now ſee it: This is ſo ſmall a pulſe of light, that it may well be queſtioned, whether the eye, aſſiſted with any artificial help, can be made ſenſible thereof: But 100 times the diſtance of a ſtar we ſee, is ſtill finite: Whence the Dr. leaves it to thoſe who conſider it attentively, to draw the concluſion.

An Account of the Method of making Sugar from the Juice of the Maple-tree *in* New England. *by Mr.* Dudley. Phil. Tranſ. N° 364. p. 27.

MAple-ſugar is made of the juice of upland maple, or maple-trees that grow upon the highlands: You box the tree as it is call'd, *i. e.* make a hole with an ax or chiſſel into the ſide of the tree, within a foot of the ground; the box you make ſhould hold about a pint; and therefore it muſt ſhelve inwards, or towards the bottom of the tree; you muſt likewiſe bark the tree above the box, to direct the juice to the box.

You muſt alſo tap the tree with a ſmall gimblet below your box, ſo as to draw the liquor off: When you have pierced or tapp'd your tree or box, you put in a reed or pipe, or a bit of cedar cut into a channel; and put a bowl, tray or ſmall caſk at the foot of the tree to receive your liquor, and ſo tend the veſſels as they are full.

After you have got your liquor, you boil it in a pot, kettle or copper: Ten gallons will make ſomewhat better than a pound of ſugar.

It becomes ſugar by the thin part evaporating in boiling; for, it muſt be boil'd, till it is as thick as treacle: Ten gallons muſt be boiled to a pint and a half.

A kettle of 20 gallons will be near 16 hours in boiling, before

before you can reduce it to three pints; a good fire may do it ſooner.

When you take it off, you muſt keep continually ſtirring it, in order to make it ſugar; otherways it will candy as hard as a rock.

Some put in a little beef-ſewet, as big as a walnut, when they take it off the fire, to make it turn the better to ſugar, and to prevent its candying; but it will do without. A good large tree will yield 20 gallons: The ſeaſon of the year for it, is from the beginning of *February* to the beginning of *April*.

Mr. *Dudley* adds, that their phyſicians look upon it not only to be as good for common uſe as the *Weſt India* ſugar, but to exceed all others for its medicinal virtue.

An Account of a Boy who liv'd a conſiderable Time without Food; by Dr. Blair. Phil. Tranſ. N° 364. p. 28.

FEB. 3. 1716. One *Gilbert Jackſon* in *Carſe-grange* in *Scotland*, about 15 years of age, fell ſick and complained of pains all over his body; and towards the latter end of the ſaid month, he was ſiez'd with a violent fever, in which he continued for three weeks and then recover'd.

The beginning of *April* following he relapſed again, his fever continuing for three weeks; and during that fever he had a ſhaking in his body as if he had been paralytic.

On the 10. of *June* following he relapſed a third time, when he became dumb, loſt his appetite entirely, and the uſe of his limbs, and continued without either eating or drinking, tho' all means were us'd to make him do both; but he recover'd of his fever on *May* 17. 1717, yet, he ſtill continued dumb, and without either eating or drinking, or having the uſe of his limbs, till the 10. of *June* the ſaid year; when he was again ſiez'd with an extraordinary fever, and the next day he recover'd his ſpeech; but continued in the fever, without either eating or drinking, or having the uſe of any of his limbs till the 11. of *November* following, when he recover'd his health pretty well and the ſtrength of one of his legs; and thus he continued without either eating or drinking, only waſhing his mouth ſometimes with water; and when he ſaw the reſt of the family go to meals it cauſed great uneaſineſs in him, and he always retir'd.

On the 10. of *June* 1718 he had a fever again, which continued till the beginning of *September* after, when he recover'd

of

of the ſaid fever, tho' he never could be prevail'd on to take any kind of meat or drink; and thus he continued in pretty good health and freſh colour'd till the 9th of *June* 1719, when he was again taken with a ſevere fever; and on the 10th at night the patient's father preſs'd him extremely to take a little milk boil'd with oatmeal, which at length he agreed to; and he took a ſpoonful of it, which ſtuck ſo long in his throat, that his parents thought he had been choak'd; and ever after he took a little food; but ſo very little, that a half-penny loaf ſerv'd him eight days: All the time he faſted, he never had any evacuation by ſtool or urine; and it was 14 or 15 days after he began to eat, that he had any; he continued in pretty good health; but wanted the uſe of one of his limbs.

This extraordinary caſe is confirm'd by the affidavit of the parents at the court of the barony of *Errol*, holden at *Errol*, *Dec.* 26. 1719, by Mr. *Charles Brown*, baily to the Right Honourable the Earl of *Northeſk*, *Gilbert Anthone* clerk, and *Charles Gill* officer.

A Method of diſcovering the Virtues of Plants *by their external Structure; by the ſame.* Phil. Tranſ. N° 364. p. 30.

THE ancients, without any of thoſe aids made uſe of by the moderns, have handed down to us ſuch an account of the virtues of plants, which are more particularly uſeful in phyſick, that all the laborious endeavours of their inquiſitive ſucceſſors have never been able to outdo them.

It muſt have been a long tract of experience, that enabled *Dioſcorides* and *Theophraſtus* to collect ſuch a laſting catalogue of the virtues of plants, as ſcarce any thing has been added to it even to this day. The Royal Academy at *Paris* have been at great pains to find out the virtues of plants by chemical analyſis, and ſeveral other experiments; of which we have the abſtracts in *Tournefort*'s *Hiſtoire des plantes aux environs de Paris*, and *Tauvry*'s *Traité des medicaments*: But theſe laborious endeavours only ſerve to confirm what the ancients have advanced, without making any new diſcovery: For, *Tournefort*, after having made the experiments with the *tourneſol* and blue paper, and given an exact account of the ſeveral active chemical principles obſerv'd in different plants, uſually concludes, *ainſi il n'eſt pas ſurprenant s'il a de telles vertues*; therefore, it is not ſurpriſing, if it is endu'd

with ſuch virtues; which is nothing but giving a reaſon, why the ancients believ'd they were good for ſuch a diſtemper.

The means us'd by our anceſtors to diſcover the virtues of plants and their uſe in the ſeveral diſeaſes, as they were the moſt ſimple, ſo they are the moſt beneficial at this very time: It ſeems they narrowly conſider'd their external appearance, and concluded, if ſuch a plant partake of ſuch virtues, ſuch another plant ſo very like it, muſt be endu'd with the ſame *ex. gr. Apium* and *Fœniculum* have the ſame manner of flowering; both produce their ſeed after the ſame mannner; their roots are both alike, being long, white, ſtreight, carnious, *&c.* Therefore, ſince a long tract of experience, handed down by tradition, ſhews, that ſuch a plant has ſuch virtues, ſuch another like it muſt have the ſame. Thus we find *Apium*, *fœniculum*, *Petroſelinum* all join'd together and preſcrib'd as opening roots in the Diſpenſatory.

This induced that expert Botaniſt, and diligent inquirer into the *Materia Medica*, Dr. *Herman*, to lay down theſe general maxims, *Quæcunque flore et ſemine conveniunt eaſdem poſſident virtutes:* And, *omnia ſemina ſtriata ſunt carminativa.*

Mr. *Petiver* obſerves, that the *plantæ umbelliferæ*, *galeatæ*, *verticillatæ*, *tetrapetalæ*, *ſiliquoſæ* and *ſiliculoſæ*, for the moſt part, have the ſame virtue and uſe; and in a letter to the Dr. he obſerves, that the *plantæ flore ſtamineo*, which he calls blink-flowers, as hops, nettles, docks, ſorrels, beets, blites, ſpinage, oraches, *bonus Henricus* or *Engliſh* mercury, and *Kali minus album*, are all good ſallads, raw or boil'd; as alſo the *leguminoſæ*, or pea-kind, as peaſe, beans, *phaſeoli* are good nouriſhing food for men; and tares, trefoils, *medicæ*, *loti* and ſaintfoins good fodder for cattle: To theſe he adds the *frumentaceæ* or *cereales*; as wheat, rye, and oats in *Europe*; and the maiz, millet, panick and ſorgum in the *Indies*, make good bread; and that from barley and rice we have good fermented and ſpirituous liquors. To theſe he adds, that the *iris* or flag-kind in foreign parts afford us drugs of no mean virtue and uſe; as ginger, galingal, turmeric, zedoary, caſumuniar and cardamums: The *laurus* or bay-kind has ſome noble attendants of the ſame tribe with itſelf; as *cinnamon*, *caſſia lignea*, *malabathrum*, *folium Indicum*, and the *camphire* tree.

In an anſwer to this the Dr. added, that all the *pappeſcentes* and *lacteſcentes*, as the *ſonchus*, *dens leonis*, *hieracium*, *lactuca*

tuca, *cichoreum*, *endivia*, *tragopogon* and *ſcorzonera* have the ſame virtues, and ſerve for the ſame uſes both in the kitchen and ſhops: All the *aſperifoliæ*, as *borago* and *bugloſſum* are call'd coolers in a more or leſs intenſe degree; for, ſome are aſtringent, as *conſolida*; others narcotic, as *cynogloſſum*. All the *galeatæ* and *labiatæ* for the moſt part conſiſt of ſubtile particles, and are therefore, cephalics, as *lavendula*, *roſmarinus*, *majorana*, &c. *mentha*, *pulegium*, *meliſſa* are hyſterics; *Salvia*, *horminum*, &c. have an attenuating virtue: And a fourth ſort, as *bugula*, *lamium*, &c. are ſomewhat aſtringent; ſo that by having an idea of the virtues of a *majorana*, *mentha*, *ſalvia*, *lamium*, we come to know the virtues of all of the ſame tribe. All the *papavera* are narcotic: The *eſulæ* and *tithymali* are cathartic; tho' both theſe are *lacteſcentes*, yet they differ from the *pappeſcentes*: All the *malvæ* are chiefly emollient: The pentaphyllous kind are aſtringent; as alſo the plantains; the corymbiferous kind, are either ſtomachics, hyſterics and vermifuges: The gentian, bitters, ſtomachics, hyſterics, febrifuges: The *pomiferæ ſcandentes*, as cucumbers, melons, &c. are coolers; but ſome are cathartic, as *cucumis ſylveſtris* and *colocynthis*. The *convolvuli*, as *mechoacanna*, &c. are purgative; to which *jalappa*, both in flower and fruit, is near of kin: *Digitalis* and *gratiola* are emetic and purgative: The ſquamous and bulbous roots are emollient, and more or leſs acrid: Thus *allium*, *cepa*, *porrum*, unboil'd, are hot, diuretic and lithontriptic: All the *ſeda* are coolers.

Thus at the firſt view, without knowing the characteriſtics or peculiar marks ſo nicely as Botaniſts do; but only exactly obſerving the external figure of the plant, when the virtue of one ſpecies is known, the virtues of all of the ſame tribe may be gueſs'd at, if not fully determined.

The next ſimple method of the ancients to diſcover the virtues of plants ſeems to have been the taſte and ſmell: Thus *apium* and *petroſelinum* have a like taſte; therefore, they are to be preſcrib'd together: The ſeeds of *fœniculum* and *aniſum* have much the ſame taſte and ſmell; and therefore, both of them muſt be carminative or expellers of wind, &c. The ancients had likewiſe recourſe to the temperament and qualities; ſuch as hot and dry, cold and moiſt, in the 1, 2, 3 and 4th degrees: But ſince the taſte is not always the ſame in one perſon, and that different perſons have different ſenſations; this, as being too much ſubjected to the different tempers and imaginations of people, is deſervedly exploded.

The Dr. compos'd a compendious ſcheme of all the plants us'd in phyſick; in which, that it might be leſs liable to objection, and that he might not ſeem to introduce any innovation in the diſtribution, he has not ſo ſtrictly obſerv'd the making their peculiar marks and virtues agree, as the diſtributing them according to their operations.

The firſt diſtribution is, by joining together all thoſe that are preſcrib'd under one title in the ſhops; as the opening roots, emollient and capillary herbs, cordial flowers, hot and cold; and the greater and leſſer ſeeds: In this the Dr. has not kept to the Diſpenſatory catalogue; but added ſeveral of the ſame tribe, that he might give a ſpecimen of what is propos'd concerning their virtues and characteriſtics: Thus, he has added *cuminum* and *meum* to *fœniculum*; *laurus Alexandrina* and *hippogloſſum* to *ruſcus*; *alcea* to *malva* and *althea*: *bonus Henricus*, *atriplex*, &c. to *beta*, under the title of *oleraceous emollients*; *lingua cervina*, *polypodium*, &c. to the capillary herbs; and ſo on in the cordial flowers, and in the hot and cold ſeeds.

The Dr. has in the ſecond place diſtributed the plants into ſuch as are alterants and evacuants: The alterants are divided into ſuch as conſiſt of groſs, and into ſuch as are ſaid to conſiſt of ſubtile particles: Thoſe conſiſting of groſs particles are aſtringent, preventing abortion and ruptures, ſtopping the immoderate flux of the *menſes*, the *fluor albus*, *diarrhœa*, *dyſentery*; and are good in burnings, bruiſes, cancers, ſpitting of blood: Groſs medicines are narcotics, vulneraries, good for ſcrophulous tumours, ſquinancy, and are coolers.

Plants conſiſting of ſubtile particles are aperient; ſuch are all opthamies, arthritics, nephritics, lithontriptics, diuretics and hydropics: They are alſo pectoral, anti-apoplectic, paralytic, hyſteric, hypocondriac, promoters of the birth, febrifuges, ſcorbutics, ſtomachics, vermifuges.

The evacuating medicines are emetic, or ſuch as work upwards; or laxative and purgative, ſuch as work downwards: The nutritive medicines are the *plantæ cereales* and *leguminoſæ*.

Here it to be obſerv'd, that the Dr. has not inſerted any plant in this table, but ſuch as are natives of *Britain*, or ſuch as are cultivated in *Britiſh* gardens; and to render it ſtill the more uſeful, he has added ſuch particular parts, as are us'd in the ſhops, *viz.* the root, herbs, leaves, tops, flowers, fruit, nuts, bark and wood.

Having thus reduced within a ſmall compaſs the moſt conſiderable virtues of plants, both general and ſpecific; and ſhewn

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the moſt eaſy, ſimple, and natural method of diſcovering them, the Dr. would not be ſo far miſunderſtood, as if he were averſe from uſing other experiments to find them out; on the contrary, he could heartily recommend another method, hitherto much neglected, and which he is perſuaded would be of conſiderable uſe, if accurately gone about; and that is their infuſion in different liquors, in order to find out the proper *menſtruum* for extracting their more uſeful parts.

Every phyſician is ſenſible, that there are ſeveral ſimples and theſe ſpecific too, which exhibited in ſubſtance, are of great efficacy; whereas if their contexture be diſſolv'd, their parts can never be ſo re-united, as to produce the ſame effect: Thus *cortex Peruvianus* is never ſo effectual, as when adminiſtred in powder: That there are others which will communicate their uſeful particles, when infus'd, to one liquor and not to another; and that the ſame ſubſtance will impregnate two liquors differently, according to the different *menſtruums*. That expert chemiſt M. *Lemery* adviſes to infuſe *opium* in water and ſpirits of wine ſeparately; and afterwards to mix both infuſions together, in order to make the *laudanum* or extract; very well conſidering, that the water will be impregnated by the more ſoluble ſaline particles, whereas the ſpirits will only imbibe the more reſinous; for, water is the proper *menſtruum* for a ſaline ſubſtance, which will not diſſolve in ſpirits of wine; this rather hardening and preſerving it from being diſſolv'd, either by air or water: Thus the moſt convenient way to preſerve the volatile ſalts of animals, is to keep them in brandy; and every one knows, that water immediately diſſolves ſugar, which brandy will not do: Therefore, *ſenna* will communicate its purgative quality to water or ale, having its ſaline particles more diſengaged; but the purgative quality of *jallop* conſiſting in its raſin, requires wine or brandy for the *menſtruum* or diſſolvent.

Therefore, Dr. *Blair* ſuppoſes, that having recourſe to the proper *menſtruums*, is a moſt proper means of finding out the virtues of plants: A ſimple may be infus'd in rain-water, ſnow-water or pure fountain-water; if its texture be looſe, and it abound in ſaline particles, thoſe pure elements will be impregnated thereby; but if the texture be more compact, firm and ſolid, and if its particles be more fix'd, it muſt be infus'd in mineral waters; or by the addition of a proportional quantity of the fix'd ſalt of a plant, a proper *menſtruum* may be prepar'd: And next to the exhibiting of bitters in ſubſtance, as wormwood, gentian, and camomile flowers, this is the

moſt

moſt convenient way of adminiſtering them; not but that their tincture extracted by brandy or wine may do very well: But ſince they greatly abound in a fix'd ſalt, a great deal of their virtue may be communicated to a leſs ſpirituous liquor, when a more ſpirituous will not extract it: The proper means to know which *menſtruum* will beſt extract the more uſeful parts of any ſimple, or rather ſuſpend its more ſolid particles, is to uſe the hydroſtatical balance; when weighing the *menſtruum* before infuſion, and after the matter has been infus'd for ſome time, it will ſoon be obſerv'd by the increaſe of the weight, how far the *menſtruum* is impregnated, and which is the moſt proper diſſolvent: The propereſt method of exhibiting the fix'd ſimples, if not in ſubſtance, is by decoction, infuſion or tincture (*N. B.* It is call'd infuſion, when the *menſtruum* is either water, wine or ale, but a tincture when brandy is made uſe of) and the beſt way to obtain the uſeful particles of volatile or ſubtile ſubſtances is by diſtillation. Theſe may, it is true, be proper ingredients for an infuſion or tincture: But there are a great many fix'd ſubſtances as improper for diſtillation, as the volatile are improper for extracts. Thus the Dr. has ſhewn the means of diſcovering the virtues of plants, without diſſolving their texture: But if any have a mind to do it by chemical analyſis, this is not to diſſuade them.

A Caſe in Surgery that is commonly miſtaken for a Fracture of the Patella; *by Mr.* Deverel. Phil. Tranſ. N° 365. p. 44.

ONE *Richard Burt* was thrown from his horſe, and in the fall receiv'd ſuch a hurt in one of his knees, as made him uncapable of remounting: He felt ſomewhat crack in that knee (as he expreſs'd it) before it touch'd the ground. Upon examining the part, Mr. *Deverel* found (as he then thought) the ends of the fractured bone drawn upwards of four fingers diſtance from each other: But upon a ſtricter examination of the parts, he found the *patella* (which was drawn upwards by the extenſors of the leg) retain'd its natural figure, and that the hardneſs which was felt below was the end of the torn ligament that ties it to the *tibia*: The ends of the ligament were brought as near as poſſible, and kept ſo about three weeks without any remarkable accident: He then began to walk, which was a little too ſoon, cauſing thereby ſome pains, and looſening the cicatrice, which made it the longer before it was perfectly firm; however he afterwards walk'd without

any

any perceivable lameneſs. Mr. *Deverel* met with two others in the ſame caſe, one of whom did not walk ſo well as ſhe uſed; tho' all imaginable care was taken: For, it is hardly to be expected, that one in ten, to whom this accident happens, ſhould ever go right; it being next to impoſſible, that the ends of the torn ligament ſhould be ſo exactly placed and retained, as not to lie over each other.

Parée in *ch.* 22. book 15 affirms, that he never ſaw any who had this bone fractured, that did not halt ever after: To this Mr. *Deverell* agrees, but he differs from him in his notions of the caſe, and the cauſe of the lameneſs. *Hildanus* in his *Obſ. Chirurg. Cent.* 5. *Obſ.* 88. *p. m.* 485. has given us an account of a tranſverſe fracture of this bone, which, after all the ſymptoms were removed, was cured; 'but, ſays he, a lameneſs and 'a great weakneſs of the whole leg enſued; ſo that the pa-'tient could not walk but with the utmoſt difficulty.' He, afterwards mentioning the cauſe of the lameneſs, queries, whether what *Parée* ſays in his *Exc. lib. de Oſſibus lib.* 4. *cap.* 2. can be the cauſe of the lameneſs; his words are to the following purpoſe; 'but tho' I ſhall not attempt to call in queſtion 'the juſtneſs of this opinion, yet there remains one difficulty, 'and that is, whether when the *patella* is fractured, the pro-'tuberance of the *callus* can be ſuch, as to fill up this cavity '(which is very conſiderable between the *femur* and *Os tibiæ*) 'ſo as to hinder the motion and action of the knee? For, we 'obſerve for the moſt part in other fractures of bones (unleſs 'there happen a very great contuſion of the bone and *perio-'ſteum*) that nature does ſo curiouſly knit the bones together, 'that there is ſeldom any veſtige of the fracture: For, the '*perioſteum*, while entire, hinders the matter of the *callus* 'from becoming an excreſcence, unleſs, *&c.* Farther in the 'caſe of the preſent patient it will appear by what follows, 'that the *callus* was not the cauſe of the lameneſs; where-'fore, we muſt here make a diſtinction.' After this he goes on to ſhew, how many different ways this bone may be fractured, but he does not obſerve, that he ever ſaw it fractured any other way (unleſs in a gun-ſhot-wound) than tranſverſely.

Ruyſch in his *Centuria Obſ. Anatomico-Chirurg.* 4. *Obſ.* 3. writes to the following purpoſe; 'ſome obſtinately affirm, 'that the *patella* cannot be fractured by reaſon of its hardneſs; 'but I have found it fractured not only by a great fall, but 'without any; a pretty robuſt man coming down from the 'end of a bridge had almoſt fallen to the ground by one foot

ſlipping,

'slipping, but recovering himself he did not come to the 'ground; however in the struggle his *patella* was fractured 'transversely, and that so plainly, that one's hand could go 'between the fracture; for one part might be felt above, the 'other below the knee.'

This observation is in all its circumstances so like that mentioned above by Mr. *Deverel*, especially, in that the fracture was made without a fall, that he is very apt to believe it is the very same; and Mr. *Deverel* supposes *Ruysch* might be deceived by mistaking the hard extremity of the ligament for the fractured bone.

And when these sorts of accidents, which are now called fractures of the *patella*, come to be nicely examined; they will perhaps be found to be generally of the same nature.

An Account of the Antiquity of the Venereal Disease; *by* Mr. Becket. Phil. Trans. N° 365. p. 47.

MR. *Becket*, before he comes to prove, that the venereal disease, when it came to be confirmed, was frequently known in *England* some hundreds of years before the siege of *Naples*, endeavours to refute the opinion of such, who take it to have had its rise there. It is true, there have not been wanting several modern authors who have asserted it; but Mr. *Becket* makes it appear to be an error as inconsiderately and hastily received, as chimerically started by some author; who, because several writers about that time, observing the disease to begin in the *pudenda*, distinguished it from another, with which it was before confounded, must likewise assert its being a new distemper, and assign a certain time and place for its rise: Now one might with all the reason in the world expect, that if the disease had its original there, it must have been so certainly and infallibly known, that there could have been no uncertain opinions about it; but that the physicians who resided in or near the place, and those more especially who interested themselves so far as to write of it, must have all of them to a man agreed upon the certainty of a thing, the truth of which was so easily attainable. But on the contrary, *Nicolas Leonicenus*, the first *Italian* physician that wrote of this disease, and who lived at the very time, when *Naples* was besieged, is so far from acknowledging it to have had its rise there, from the *French* soldiers conversation with the *Italian* women, that he does not allow it to be the consequence of impure embraces, so little did he know of its true cause. It

was

was likewiſe about this time that Pope *Alexander* VI. engaged *Jaſper Torella* to write of this diſtemper; who is ſo far from allowing it to have had its original there, that he tells us, the aſtrologers were of opinion, it proceeded from he knows not what particular conſtellations: Neither does *Sebaſtianus Aquilanus*, who liv'd at that time, or *Antonius Scarolius*, who wrote in 1498, about four or five years after the ſiege, allow it to be any other than an ancient diſeaſe: Nor do ſeveral other authors, then living, mention one word of this *Neapolitan* ſtory. But it ſeems *Ulricus de Hutten* a *German* knight, who was no phyſician, poſitively affirms this diſeaſe to have had its riſe there; but how he ſhould come to know this, who lived at ſuch a diſtance from the place; and how they, who were phyſicians, reſiding, as it were, on the ſpot, ſhould be ignorant of it, will be as much credited, as his following inconſiſtent relation. He tells us this diſeaſe was unknown till 1493, or thereabouts; that he himſelf had it when a child; and conſequently, that it was hereditary, or from the nurſe. He wrote his book of this diſtemper at *Mentz*, where it was printed by *John Scheffer* in *quarto* in 1519. Now if we allow him to be but 27 years of age, when he wrote (for, he cannot be ſuppoſed to be leſs, who before this time took upon him to cure his father of the venereal diſeaſe, without the aſſiſtance of any phyſician or ſurgeon) he muſt have had the diſtemper upon him, according to his own account, before ever it was in being.

In *Phil. Tranſ.* N° 357. Mr. *Becket* thinks he has ſufficiently proved, that the firſt degree of the venereal diſeaſe was very common here in *England*, ſome hundreds of years before it is commonly ſaid to have been known in *Europe*; conſequently, there will be no reaſon to ſuppoſe we were ſtrangers to it, when it came to be confirmed abroad: Now when it was in this confirmed ſtate, the writers of thoſe early times looked upon it as an entirely new diſeaſe, and not a conſequence of any diſorder before contracted; becauſe they were not appris'd, that, the firſt ſymptoms being removed, and the diſeaſe in appearance cured, it ſhould afterwards diſcover itſelf in ſuch a manner, as ſhould not ſeem to have the leaſt analogy with the ſymptoms, that firſt attacked a part, which had for a conſiderable time been free from any misfortune. But becauſe the ſymptoms are the only true characteriſtics, whereby we can infallibly diſtinguiſh one diſeaſe from another: Mr. *Becket* produces ſufficient authorities to ſhew, that the ſymptoms were

all of them known and deſcribed by ancient phyſical and chirurgical writers, juſt as they appear in the venereal diſeaſe at this day.

Mr. *Becket* thinks he has in *Phil. Tranſ.* N° 357. made it ſufficiently appear, that the firſt degree of this diſeaſe was anciently known in *England* by the name of the *brenning* or *burning*; and that it was the ſame thing with what we now call a *clap*. The ſymptoms, which are uſually its concomitants, are the *phymoſis* and *paraphymoſis*, both which are accurately deſcribed, with proper remedies for them, by Mr. *John Arden* in a manuſcript of his, curiouſly written on *vellum* and beautifully illuminated. The imprudent method of cure of this firſt degree of the venereal malady is ſometimes attended with a caruncle in the *urethra*, which was anciently a diſeaſe very common in *England:* For, not to mention other early writers, the above-mentioned author gives us the caſe of a certain Rector, that had a ſubſtance, reſembling a wart, growing in the *penis*; which in another place he ſays frequently happens; and of another, who had ſuch an excreſcence, as big as a ſmall ſtrawberry, which, ſays he, proceeded from the purulent matter that remained in the *urethra*.

And indeed, there is not any ſymptom of the venereal diſeaſe Mr. *Becket* finds ſo often mentioned, as this of the caruncle; inſomuch that it ſeems to have been more common in thoſe early times than at this day: But this muſt be certainly owing to the ſmooth and oily remedies they were continually injecting, which by their relaxing and ſoftening the fibres of the part, muſt neceſſarily diſpoſe the contexture of the ſmall blood-veſſels, lodged at the bottom of the little ulcerations, to fill with nutritious juices and to extend in ſuch manner, as to form ſuch fungous excreſcencies; and ſo ſolicitous were they to remove theſe inconveniencies, that they made uſe of ſeveral ways by corroſives and other methods to accompliſh this end; and a very early writer among us has given a very methodical and curious tract on this ſubject, wherein he recommends the removing them by the medicated candle, which we uſe at this day; and lays down divers other inſtructions in relation thereto; which, probably, makes it the beſt diſcourſe on this ſubject that was ever yet written. The ſame author takes notice of thoſe obſtinate ulcers, that happen on the *glans* and the neighbouring parts, now called ſhankers; and the great trouble our ancient authors found in attempting their cure ſufficiently ſhew them to have had their original from a venereal infection.

Our

Our early authors are very full in their accounts of these several symptoms of the venereal disease, and of others, when the disease was in a more confirmed state, to which they appropriated particular names, perhaps more significant and expressive than those imposed by modern authors: Thus for instance, the *bubo*'s in the groin they called *dorsers*, for which a reason was assigned *Phil. Trans.* N° 357; and the venereal nodes on the shin-bones they called the *boonhaw*, which gives us a perfect idea, not only of the part affected, but after what manner it was diseased; for, the old *English* word *hawe* signified a swelling of any part: Thus for instance, a little swelling on the *cornea*, was anciently called the *hawe* in the eye, and the swelling that frequently happens on the finger on one side the nail, was called the white *hawe*, and afterwards *whitflaw* or whitlow. The process the last mentioned author recommends for curing the *boon* or *bone-hawe* is to use a plaister, which had a hole cut in the middle to circumscribe it; and applying a caustic of unslacked lime and black soap incorporated together, which, with the plaister and bandage, was to be fastened on the part four hours or longer; after this he proceeds to separate the slough, *&c.* This practice of his seems to have been found out by accident. For, he tells us, when he was a young practitioner, having applied both the natural and artificial arsenic to a patient's leg, it mortified the flesh in a surprising manner; but the eschar coming off by proper digestives, and leaving the bone bare, he scraped it with an instrument for several days, and dressed it with incarnatives; but this proving unsuccessful, he continued to scrape it till he observed it to move under the instrument; after which having separated it, he found the sore covered with new flesh, and that the bone was four inches in length, two in breadth and very thick; upon the removal of which the patient was soon cured.

Thus it is probable, that this observation of this great man led our predecessors to practise the very same method; and we do at this day in our hospitals treat the venereal nodes on the shins exactly as is here described; where we observe the same appearances, he so long ago took notice of; and it is not in the least to be doubted, but the *boonhaw* and our venereal nodes are the same disease. By the appearance of some of the last of the above-mentioned symptoms, we infallibly judge the patient has had the infection upon him a considerable time, and that the disease is making its gradual advances, to the corrupting and destroying the whole frame of the body: That this was the conclusion

of the miſeries of thoſe, who gave themſelves up to the embraces of lewd women, in thoſe early times as well as now, is evident from ſome remarkable inſtances in a M. S. (in *Lincoln College Oxon.*) writ and collected by one Mr. *Tho. Gaſcoigne* a Dr. of divinity, whoſe words are ' *Novi enim ego Magiſter Thomas Gaſ-* ' *coigne, licet indignus, ſacræ theologiæ Doctor, qui hæc ſcripſi & col-* ' *legi, diverſos viros, qui mortui fuerunt ex putrefactione membrorum* ' *ſuorum genitalium & corporis ſui; quæ corruptio & putrefactio, ut* ' *ipſi dixerunt, cauſata fuit per exercitium copulæ carnalis cum Mulie-* ' *ribus. Magnus enim Dux in Anglia, Scil. J. de Gaunt, mortuus eſt* ' *ex tali putrefactione membrorum genitalium & corporis ſui, cauſata per* ' *frequentationem mulierum. Magnus enim fornicator fuit, ut in toto* ' *regno Angliæ divulgabatur, & ante mortem ſuam jacens ſic infirmus in* ' *lecto, eandem putrefactionem Regi Angliæ Ricardo ſecundo oſtendit, cum* ' *idem Rex eundem Ducem in ſua infirmitate viſitavit, & dixit mihi,* ' *qui iſta novit unus fidelis ſacræ Theologiæ Baccalaureus. Willus* ' *etiam longe vir maturæ ætatis, & de Civitat. Lond. mortuus eſt ex* ' *tali putrefactione membrorum ſuorum genitalium & corporis ſui, cauſatâ* ' *per copulam carnalem cum mulieribus, ut ipſemet pluries confeſſus eſt ante* ' *mortem ſuam, quum manu ſuâ propriâ eleemoſynas diſtribuit, ut ego novi,* ' *An. Dni.* 1439.'

Now that thoſe inſtances from *Arden*, and thoſe from *Gaſcoigne*, who was then Chancellor of *Oxford*, could poſſibly be no other than venereal caſes, is plain: Certain it is, that no other diſeaſe is got by carnal converſation with women, which firſt attacks the genitals, cauſing a corruption and putrifaction of them, and afterwards of the whole frame of the body, but that which is venereal: For, nothing is more commonly known at this day than that after venereal embraces with an impure woman, the *penis* is the part where the ſcene is firſt laid for the ſucceeding tragical appearances; and there and in the neighbouring parts, do the ſymptoms of the diſeaſe, as its retainers, always firſt aſſemble; till the malignant poiſon taint the blood and other juices; which being conveyed over the whole frame of the human fabrick, if not check'd, ſoon brings about its total putrefaction.

What Mr. *Becket* further adds in relation to this is, ſince we do not find, that the diſeaſe mentioned by *Gaſcoigne* was diſtinguiſhed by any particular name; and that great numbers muſt have unavoidably died of it, at that time, from the imperfect knowledge of thoſe who had the treatment of the firſt degrees of it; it muſt neceſſarily follow, that when the whole frame of the body had received a taint from the venereal poiſon; ſo as to occaſion its breaking out into ſcabs and ulcers all over its ſurface, it muſt commonly be called by the name of ſome particular diſeaſe, whoſe appearances had ſomewhat of an affinity to it. Now if

we

we examine the nature of all the diſeaſes that attack the human body, we ſhall not find the venereal malady, when it arrives at this ſtate, to bear a greater reſemblance to any other than the leproſy, as deſcrib'd by the ancients: Nay, ſo great was the analogy between theſe diſeaſes, that *Sebaſtianus Aquilanus* has endeavour'd to prove from *Galen*, *Avicen*, *Pliny*, &c. that the pox is only one ſpecies of the leproſy; and *Jacobus Cataneus*, a writer almoſt as early as the riſe of the name of the pox, tells us, that it is not only poſſible there may be a tranſition from one of theſe diſeaſes to the other; but that he ſaw two perſons in whom the pox was changed into the leproſy; that is, from having large puſtules on the ſurface of their bodies (from which the pox is denominated) to have become ulcerous or ſcabby. This particular ſtate of the diſeaſe anciently put ſurgeons to a great deal of trouble: For, finding, that theſe ulcers were of a very obſtinate nature, they were oblig'd to make uſe of great numbers of remedies, in order to conquer their bad diſpoſition: But they obſerv'd, that all of them were uſeleſs, unleſs mercury was join'd with them: Now the dreſſing each particular ulcer being ſo very tedious, they order'd the patients to daub the ointments over the parts which were ulcerated; which done they were wrapp'd in linen cloaths till the next dreſſing: But after a few days they were extremely ſurpris'd to find their mouths begin to be ſore, and that they ſpit very plentifully; but they tell us to their ſurpriſe, that in a little time the ſores heal'd and the patients were cur'd: And by this accident it was, that the method of ſalivating by unction was firſt diſcovered, which is ſo much uſed among us at this day.

Now, tho' thoſe foreign authorities above-mentioned might be look'd on as ſufficient to prove, how our anceſtors confounded theſe two diſeaſes together; yet he proves from our own writers long before thoſe, that tho' the pox was not only among us, but in diſtant nations anciently confounded with the leproſy; yet ſo exact were our writers in their obſervations of the infectious nature of one ſpecies of that diſeaſe, and in deſcribing the ſymptoms, as was ſufficient to enable any one to diſtinguiſh between them: He therefore, firſt inquires into the manner, how the leproſy was ſometimes ſaid to be gotten in thoſe early times, and in the next place he examines the ſymptoms of the diſeaſe that attack'd the patient.

John Gadiſden a very learned and famous phyſician, who flouriſh'd about the year 1340, in an excellent work of his entitled

entitled *Rosa Anglica*, speaking *de infectione ex coitu leprosi vel leprosæ*, has the following words; *primò notandum, quod ille qui timet de excoriatione & arsurâ virgæ post coitum statim lavet virgam cum aquâ mixtâ aceto vel urinâ propri intra vel extra præputium*: Speaking of the leprosy, he likewise recommends a decoction of plantain and roses in wine to be made use of by the woman, immediately after the venereal encounter; upon which he tells us she will be secure. Hence it is evident, that some of their leprous women (as they call'd them) could communicate an infectious malady to such as had carnal conversation with them; which proves, that the *pudenda* of the women must be diseas'd; forasmuch as we are absolutely assur'd, that infections of that nature only happen, when a sound part comes to an immediate contact with a diseas'd one; for, the symptoms always display themselves first in those parts thro' which the virulency is first convey'd. Now in a true leprosy we never meet with the mention of any disorder in those parts; which, if there be not, must absolutely secure the person from having that disease communicated to him by coition with leprous women; but it proves, there was a disease among them, which was not the leprosy, tho' it went by that name, and that this could be no other than venereal, because it was infectious: For, there is no other disease that can be communicated this way but the veneral; seeing the *pudenda* are only in that distemper so diseased, as to become capable of communicating their contagion.

The learned *Gilbertus Anglicus*, who flourish'd about the year 1360, reasons concerning the manner, how it is possible a man should be infected by a leprous woman; where if we allow him to call the malignant matter, which is lodged in the *vagina* (the *semen fœmininum*) we shall find, that he accurately describes the very first venereal infection, by part of the virulent matter being receiv'd into the *urethra*; from whence by means of the veins and arteries it is convey'd into the whole body, after which, says he, ensues its total corruption.

Let us now examine the symptoms of one sort of their leprosy; for, it must necessarily be divided into different species, when another distemper was blended with it, in which we observe such a diversity of appearances; and this Mr. *Becket* the rather does in this place, because it will furnish us with the next succession of symptoms after those already mentioned, as the venereal *ozæna's*, ulcers of the throat, hoarseness, the

proof

proof of its being communicable from the nurſe to the child, by hereditary ſucceſſion, *&c.* All which we find to be true in the venereal diſeaſe at this day.

Our countryman *Bartholomew Glanvile*, who flouriſh'd about the year 1360, in his book *De proprietatibus rerum*, tranſlated by *John Treviſa* Vicar of *Barkley* in 1398 tells us, 'ſome leprous perſons have redde pymples and whelkes in the 'face, out of whome oftenne runne blood and matter: In 'ſuch the noſes ſwellen and ben grete, the vertue of ſmellynge 'faylyth, and the brethe ſtynkyth right fowle.' In another place the ſame author ſpeaks of 'unclene ſpoty'd glemy and 'quyttery, the noſethrilles ben ſtopy'd, the waſen of the 'voys is rough, and the voyce is horſe and the heere falls.' Among the cauſes of this ſort of leproſy, he reckons lying in the ſheets after them, eaſing nature after them and others, which the firſt writers on the pox look'd upon to be capable of communicating that contagion; 'alſo, ſays he, it comyth of 'fleſhly lyking by a woman, after that a leprous man hathe 'laye by her; alſo it comyth of fader and moder; and ſo this 'contagyon paſſyth into the chylde, as it ware by lawe of 'herytage; and alſo when a chylde is fedde wyth corrupte 'mylke of a leprouſe nouryce.' He adds, 'by whatever cauſe 'it comes, you are not to hope for cure, if it be confyrm'd; 'but it may be ſomewhat hidde and lett that it deſtroye ſo 'ſoone.'

Thus we ſee how our author, under the name of one ſpecies of the leproſy, gives a ſummary of the ſymptoms of the pox, and the ſeveral ways by which it is at this time communicated. Now when theſe two diſeaſes were anciently blended together, and paſs'd under the name of the leproſy only, this muſt be the real cauſe, why that diſeaſe ſeem'd to be ſo rife formerly; for, two diſtempers paſſing under one name muſt neceſſarily make it more taken notice of and much more frequent; not but that much the greater number of thoſe who were formerly ſaid to be leprous were really venereal, ſeems to be very evident; for, ſince that diſeaſe has been diſtinguiſh'd from the leproſy, it has drawn off ſuch vaſt numbers, that the leproſy is become a perfect ſtranger among us.

Thoſe acquainted with our *Engliſh* hiſtory, well know the great proviſion that was anciently made throughout all *England* for leprous perſons; inſomuch that there was ſcarce a conſiderable town among us but had a lazar-houſe for ſuch patients. In a regiſter belonging to one of theſe houſes,

Mr.

Mr. *Becket* finds, that in *Henry* VIIIths time there were ſix of them near *London*, *viz.* at *Knightsbridge*, *Hammerſmith*, *Highgate*, *Kingſland*, the *Lock*, and at *Mile-end*; but about 40 years before, he finds but four mentioned; and in 1452 in the will of *Ralph Holland* merchant taylor, regiſter'd in the prerogative office, mention is made but of three, which, with his legacies to them, are as follows. *Item lego leproſis de* Lokes *extra barram Sti* Georgii 20 s. *Item lego leproſis de* Hackenay (which is that at *Kingſland*) 20 s. *Item lego leproſis Sti* Egidii *extra de* Holborn, 40 s. From which it is worth while to obſerve, that the *Lock* beyond St. *George*'s church and that at *Kingſland*, are at this time applied to no other uſe than for the entertainment and cure of ſuch as have the venereal malady.

Some of our learned antiquaries have been much at a loſs to know the cauſe, why the leproſy ſhould be ſo common in thoſe early times, and ſo little known among us now: But Mr. *Becket* thinks it will be impoſſible to aſſign the reaſon, unleſs we allow, according to the proofs he has already adduced, that the venereal diſeaſe was ſo blended with it, as to make up the number of the diſeaſed. It ſeems to have been the ſame thing with them in *France* as with us in *England:* For, the author of the hiſtory of that kingdom, publiſhed in *England* in 2 vol. *Octavo*, tells us, that the houſe of the fathers of the miſſion of St. *Lazarus* was formerly an hoſpital for leprous people; but that diſeaſe having ceaſed in this laſt age (ſince the pox has been diſtinguiſhed from it) theſe lazar-houſes have been converted to other uſes; and Mr. *Becket* obſerves, that the writ *de leproſo amovendo*, contained in the regiſter of writs was (according to *Coke* upon *Littleton*) to prevent leprous perſons aſſociating themſelves with their neighbours, who appeared to be ſo by their voice, their ſores, the putrifaction of their fleſh, and their ſtench. The method taken to prevent this noiſome and filthy diſtemper, the leproſy, was caſtration which would infallibly prevent their getting the pox in the uſual manner: It is certain that eunuchs are rarely or never troubled with the leproſy, according to M. *Le Preſtre*, a Counſellor in the Parliament of *Paris*, who in *Cent.* 1. *cap.* 6. *de ſeparatione ex cauſâ luis venereæ* writes to the following purpoſe, 'antipathy reſiſts the poiſon of the leproſy; 'hence eunuchs and ſuch as are of a ſoft, cold and effeminate 'nature are ſeldom or never infected with the leproſy; and ſuch 'as are in danger of it, may, according to the opinion of phy-'ſicians, be caſtrated.' And *Mezeray* ſays, he has read in the life of *Philip* the *Auguſt*, that ſome men had ſuch apprehen-

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ſions of the leproſy (that ſhameful and naſty diſtemper) that to preſerve themſelves therefrom, they made themſelves eunuchs. Now it is highly probable, that thoſe who ſubmitted to ſo painful an operation, having before obſerved, that ſuch, as gave themſelves up to a free and unreſtrained uſe of women, fell at length under ſuch unhappy circumſtances, found that to be the only means to preſerve themſelves from it; which ſufficiently proves this ſpecies of the leproſy to be infectious; and for the reaſons before aſſign'd, it could be no other than venereal: For, how the true leproſy ſhould be prevented by ſuch means will be, Mr. *Becket* thinks, impoſſible for any perſon to determine.

There ſtill remains one very conſiderable ſymptom of the venereal malady to be taken notice of; becauſe it is looked upon to be the moſt remarkable in that diſeaſe, *viz.* the falling of the noſe: But ſince it has been already prov'd, that this diſeaſe (when it had arrived to ſuch a pitch, as to diſcover itſelf by thoſe direful ſymptoms, as are the immediate fore-runners of this) was by the ancients confounded with the leproſy, and called by that name, it muſt be among the ſymptoms of that diſeaſe we are the moſt likely to meet with it, if any ſuch thing as the falling of the noſe was known among them. Now the moſt likely method of coming to a certain knowledge of the infallible ſymptoms of the leproſy among the ancients, in its more confirmed ſtate, is to conſult the examinations thoſe unhappy perſons were obliged to undergo, before they were debarred the converſation of human ſociety and committed to cloſe confinement; and this Mr. *Becket* does from what remains he has met with in records and other ſcattered papers.

Firſt then, after the perſons, appointed to examine the diſeaſed, had comforted them, by telling them, that this diſtemper might prove a ſpiritual advantage; and if they were found to be leprous, it was to be looked upon as their purgatory in this world; and that tho' they were denied the world, they were choſen of God: The perſon ſuſpected was then to ſwear to anſwer truly to all ſuch queſtions, as ſhould be aſked. The examiners were very cautious in their enquiries, leaſt a perſon that was not really leprous ſhould be committed, which they look'd upon to be an almoſt unpardonable crime: They conſidered the ſigns as univocal, which properly belonged to that diſeaſe, or equivocal, which might belong to another; and did not upon the appearance of one or two ſigns determine the perſon to be a *Lazar*; and this Mr. *Becket* finds to have been the caſe of the wife of *John Nightingale* Eſq; of *Burntwood* in *Eſſex*, who in the

reign of *Edward* IV. *Anno* 1468 being reported to be a *Lazere*, and that ſhe converſed with perſons in public and private places, and did not (according to cuſtom) retire, but refuſed ſo to do, was accordingly examined by *William Hattecliff*, *Roger Marcal* and *Dominicus de Serego*, the king's phyſicians: But they upon ſtrict enquiry adjudged her not to be leprous, by reaſon the appearances of the diſeaſe were not ſufficient: Some of the queſtions put to the leprous perſons (as they called them) will more fully confirm what Mr. *Becket* has before advanced; he tranſcribed them from an old book of ſurgery, as follows; 'yf there were 'any of his lygnage that he knew to be *Lazares* and eſpecially 'their faders and moders; for, by any other of their kynred 'they ought not to be *Lazares*; then ought ye to enquire yf he 'hath had the company of any lepreſs woman, and yf any *La-* 'zere had medled with her afore him; and lately becauſe of 'the infect matter and contagyous filth that ſhe had receiv'd of 'hym. Alſo yf his noſtrills be wyde outward, narrow within 'and gnawn: Alſo yf his lips and gummes are fowle, ſtynking 'and coroded: Alſo yf his voice be horſe, and as he ſpeaketh in 'the noſe.' Now the ſigns, which are here mentioned, were look'd upon to be univocal, and which made the examiners principally determine the perſons to be leprous; but what determinations any one would immediately give from ſuch ſymptoms at this time, no perſon ſure is ignorant of. But even theſe certain appearances would not always ſatisfy ſome perſons, if we may credit *Fælix Platerus* in his medical and chirurgical *obſ. lib.* 3. who tells us, that ſome did not look upon them to be ſo, till the patients had an horrible aſpect, were hoarſe and their noſes fallen: Likewiſe in the *Exâmen leproſorum*, printed in the *De Chirurgia ſcriptores Optimi*, the author, ſpeaking of the ſigns of the leproſy, relating to the noſe, begins thus, *ſi nares exterius ſecundum exteriorem partem ingroſſentur, et interius conſtringantur & coarctentur; ſecundo ſi appareat cartilaginis in medio corroſio, et caſus ejus ſignificat lepram incurabilem.* And the above-mentioned *John Gadiſden* in his *chap. de lepra* ſays as follows; *ſigna confirmationis etiam incurabiliter, ſunt corroſio cartilaginis quæ eſt inter foramina, et caſus ejuſdem.*

Thus it has been proved, we had a diſtemper among us ſome hundreds of years before the venereal diſeaſe is ſaid to have been known in *Europe*, that was called the *Burning*; that this *burning* was infectious, and the firſt degree of the venereal diſeaſe; that this being common at that time, from their method of treatment, the pox muſt be unavoidable; that it had exactly the ſame appear-

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ances it has now, tho' generally called by different names; that the ancients confounded it with the leproſy; that the vaſt number of leprous patients among us, before the venereal diſeaſe was diſtinguiſhed from it; and the ſmall number obſerv'd at this day, is a flagrant proof of the former; that in deſcribing the ſymptoms of the leproſy, they give us thoſe of the venereal malady; and by mentioning the manner of its being communicated, they deſcribe that in which the pox is communicated at this day; that they recommended the ſame remedies to prevent the firſt attack of the leproſy, as are at this day in uſe to prevent the firſt ſymptoms of the pox; and that the falling of the noſe, which is looked upon as the moſt remarkable ſymptom of the venereal diſeaſe, was anciently obſerv'd in what they call'd the leproſy in former ages.

An Account of the great Meteor that appeared on the 6th of March 1715—16; *by Mr.* Cotes. Phil. Tranſ. N° 365. p. 66.

THE appearance of the meteor was very nearly the ſame at *Cambridge* with what Dr. *Dannye* obſerv'd it at *Spofferth* in *Yorkſhire*; only that the triangular ſtreams of light were not ſo permanent; and the point to which they all converged was diſtant from the zenith about 20 degrees; its azimuth lying between the ſouth and the eaſt at about 10 degrees from the ſouth, towards which point of the compaſs the wind tended: The poſition of this point of convergence may be accurately determined; for, at a quarter after ſeven, when the appearance at *Cambridge* was in its greateſt perfection, it lay nearly in the middle between the two bright ſtars in the heads of *Caſtor* and *Pollux*. Mr. *Cotes* was inform'd, that ſome ſtreams were obſerv'd to ſhoot forth immediately after ſun-ſet, and that they did not entirely ceaſe till about three or four in the morning.

It was after ſeven before Mr. *Cotes* had notice of this uncommon ſight: At firſt he only ſaw two or three of the triangular ſtreams towards the north and north-weſt; theſe were not of a long continuance, but were ſucceeded by others which appeared and vaniſh'd again by turns, ariſing from and aſcending up to places in the heavens of very different altitudes above the horizon. From the time he began to view them, they began to aſcend more and more copiouſly, being propagated ſtill farther and farther from the north towards the weſt and eaſt, and always directed towards the heads of *Gemini*;

till at length when they ſeem'd almoſt to meet at the point of convergence, they alſo began to aſcend up towards it from the ſouthern parts and all around it; inſomuch, that at a quarter after ſeven, there was a perfect canopy of rays: The bottom of this canopy did no where reach down to the horizon; for, near the north, where it deſcended moſt, its altitude was about 10 or 15 degrees; and near the ſouth where it deſcended leaſt, its altitude was about 40 degrees: It remain'd in this ſtate about two minutes, during which time ſeveral colours were obſerv'd; ſome fainter and ſome more permanent; others brighter, but quickly vaniſhing: Thus in the weſt Mr. *Cotes* obſerv'd the rays to be tinged for ſome conſiderable time with an obſcure and heavy red; and in one of the brighteſt ſtreams at another time, there ſuddenly broke out a very vivid red, which was inſtantly and gradually ſucceeded by the other priſmatic colours, all vaniſhing in about a ſecond of time: Theſe colours affected the ſenſe ſo ſtrongly, that he thought them to be more intenſe than thoſe of the brighteſt rainbow he had ever ſeen. A little time before the appearance loſt its perfection, a ſhaking and trembling of the ſtreams was obſerv'd, chiefly in their upper parts; during which, their convergence was confounded, and the whole heaven ſeem'd to be in a convulſion: At the ſame time Mr. *Cotes* could perceive waves of light towards the north, which mov'd upwards, and in their motion croſs'd the ſtreams, lying parallel to the horizon; theſe waves were different from thoſe broad ones, obſerv'd in *Yorkſhire*, and which Mr. *Cotes* likewiſe took notice of; their breadth ſeemed to be about a degree, their length about 90 degrees; and they reſembled thoſe ſlender waves on the ſurface of ſtagnant water, caus'd by caſting in a ſmall ſtone.

About ſeven or eight years before, Mr. *Cotes* obſerv'd this other meteor; along the horizon in the north, there lay a white and luminous, and ſeemingly denſe matter in the form of a cloud, repreſented by *a b c d* Fig. 7. Plate II. its length *a b* was about 10 or 15 degrees; from this there aroſe directly upwards, pointed ſtreams of the like luminous and white matter, which yet did not appear in any part thereof to be ſo denſe as the former; and it became gradually more and more rare in its upper parts, ſo as to vaniſh almoſt inſenſibly at the points: There was ſome little difference in the height of theſe ſtreams; but they generally aſcended up to about four degrees above the horizon; they were very numerous and contiguous

guous to each other, and ſeemed to be compos'd of very ſlender parallel filaments or rays. This was the common appearance; and the only remarkable thing he farther obſerv'd was, that ſometimes a fire or flame would break out in the cloud *a b c d*, and move along it in a direction, parallel to the horizon; and during this meteor, a pointed ſtream directly over the fire ſeemed to run along with it, and to paſs by the other more fix'd ſtreams, to which it always kept itſelf parallel.

Mr. *Cotes* is perſuaded, that the appearance of *March 6.* was of the ſame kind with this; for, let A B Fig. 8. repreſent the plane of the horizon; C the place of the ſpectator; E F a fund of vapours or exhalations at a conſiderable height above us, diffus'd every way into a large and ſpacious plane, parallel to the horizon: This fund of mix'd matter will by fermentation emit ſtreams from itſelf, ſuch as E G, F H, *&c.* which, if the wind be perfectly ſtill, will aſcend perpendicularly upwards; if it be boiſterous and irregular, they will be blended and confounded together; but if it be very gentle and uniform, as it was at the time of the appearance, they will be inclin'd towards the point of the horizon, which is oppoſite to that from which the wind blows: Now if A D B repreſent the concave of the heavens; and a line C D be drawn parallel to the columns E G, F G, *&c.* it is certain by the rules of perſpective, that theſe columns will appear upon that concave to converge all around towards the point D; thus the column E G will ſeem to ariſe from the point *e*, to aſcend up to *g*, and to take up the ſpace *e g*; and in like manner the arch *f h* will be the projection of the column F H: Hence it is evident, that the reaſon why the triangular ſtreams aſcended at firſt only from the northern parts of the heavens was, that the fund of matter E F was not yet arriv'd in its motion to the line C D; after it had paſs'd that line, it is plain, they muſt appear to aſcend from all quarters: A great number of columns being, therefore, diſpos'd to emit light at the ſame time, caus'd that perfect canopy, deſcrib'd above. The reaſon why that canopy deſcended lower in the north than in the ſouth was, that the ſhining columns, which had not yet paſs'd the line C D, were more numerous and more remote from it than thoſe which had paſs'd it; for, if the point E be farther diſtant from C D than the point F, the arch A *e* muſt needs be leſs than the arch B *f*: An irregular guſt of wind blowing upon and ſhaking the columns was, Mr. *Cotes* ſuppoſes, the cauſe of that trembling, which

which appeared in the triangular ſtreams, and likewiſe the cauſe which deſtroy'd that fine appearance of the canopy: The ſlender circular waves, obſerv'd at the ſame time, might likewiſe be explained from the ſame cauſe.

In order to repreſent this appearance in ſome tolerable meaſure, take a hoop, and round about it faſten ſeveral ſtreight ſticks parallel to each other, but all inclin'd to the plane of the hoop; hold this plane parallel to the horizon, and in that poſition move it with ſticks over a candle, the ſhadows of the ſticks upon the ceiling of the room will converge to a point, not directly over the candle (as they would have done, had the ſticks been perpendicular to the plane of the hoop) but to the point, in which a line drawn from the candle, parallel to the ſticks, ſhall interſect the plane of the ceiling.

An Account of the Operation of Medicines by Dr. John Quincy. Phil. Tranſ. N° 365. p. 71.

AS to the operation of medicines and particularly of purges, Dr. *Quincy* ſuppoſes; 1. That all thoſe parts of an animal body, that are vaſcular, or thro' which any fluid paſſes from the inteſtines to the minuteſt fibre, are the ſeat of the operation of a medicine. 2. That this whole courſe of circulation or animal motion is naturally diſtinguiſh'd into three different ſtages, according to the different capacities of the veſſels, and the motions of their contents; each having its proper out-let, and that theſe are the ſeat of the three concoctions, ſo often mentioned by phyſical authors; the firſt in the ſtomach and bowels, having the *anus* for its emunctory; the ſecond all within the circulation of the blood, ſo far as it retains its colour, having the kidneys; and the third all beyond that circuit, having the ſkin for its emunctory. 3. That every medicine, which cauſes evacuation, is a purge. 4. That every purge operates as a diſſolvent, by fuſing the juices, and increaſing the quantity fit for expulſion; or as a *ſtimulus* by accelerating their motions, ſo as to bring the matter fit for expulſion oftner to the ſecretory out-let; or both.

Theſe aſſumptions are premis'd only for the better proof of this grand propoſition; *viz.* that a change in the bulks, figures and motions of the component particles of a purging medicine will change the ſeat of its operation, and fit it for exertion in larger or leſſer veſſels, as thoſe mechanical affections are intended or remitted.

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For illuſtration hereof, it may be convenient to attend the management of common practice, in making a purge operate more or leſs than it otherways would do.

Subſtances that are groſs and heavy, as thoſe chiefly conſiſting of ſaline and earthy particles, ſuch as tartar, manna, &c. if they are reduced ſmaller by triture or repeated ſolutions, operate more gently; but if ſharpened by acids or any management that expoſes their angles more ſenſibly to the membranes, they are rougher and take place ſooner.

Reſinous medicines as ſcammony, gamboge, jalap and moſt of vegetable production are more violent and operate ſooner, when they are more tenacious and adheſive, as in their extracts; but gentler, when divided by hard brittle ſubſtances, ſuch as ſalt of tartar, ſugar, &c.

Medicines, that have ſulphur and ſalt in their compoſition, are more or leſs rough and ſpeedy in their operation, in proportion to their greater or leſs participation of the ſaline ingredient and the aſperity of its angles; of this kind are moſt minerals and their preparations; and it may be ſufficient to inſtance in the management of antimony and mercury; the firſt of theſe is by chemical analyſis known to be a compoſition of a ſubtile ſulphur and ſalt, and the more the ſaline part is let looſe by preparation, and opening the ſulphur, as it is commonly called, the more vehemently and the ſooner will it operate; whereas, in its leſſer preparations, when the ſalts are cloſely wrapped up in their native ſulphur, they will hardly work at all, till they are got into the fartheſt ſtages of circulation; mercury *per ſe* is little known as a medicine; and its firſt preparation, which makes it into ſublimate, ſo loads it with ſaline *ſpicula* that it amounts even to a poiſon; but the more thoſe *ſpicula* are broke by triture, ſublimation, &c. the milder it operates; and if to the comminution of its points there be added a ſulphur ſubtile enough to join it, it may be reduced to ſo mild a medicine, as not to be felt, but in the laſt ſtage of operation.

This ſhort view may be ſufficient to ſhew, that it is exceſs of aſperity and motion in a medicine, that will not ſuffer it to paſs the ſtomach, without irritating it into ſuch convulſions, as will throw it up again by vomit; that a farther comminution and ſmoothing its figure will admit it into the bowels, and make it operate by ſtool; that a ſtill farther remiſſion of its properties will convey it into the blood, and make it there promote evacuation by urine; and that a ſtill farther comminution will convey it into the minuteſt canals, where by the ſame properties, only in a lower

a lower degree, it will increaſe perſpiration or cauſe ſweat: So that the ſubtiler medicines operate in the capillaries and ſmalleſt fibres by the ſame mechaniſm, that more groſs medicines do in the common ſtream of the blood, when they go off by urine; or the groſſeſt of all do in the larger paſſages, when they promote ſtool.

Hence the ſkill of preparing and adminiſtering medicine conſiſts in proportioning its manifeſt and known properties to the capacity and circumſtances of the part it is to operate in; and to intend or remit its mechanical affections, as it is ſooner or later to take place in the larger or ſmaller veſſels.

Of the firſt claſs there are few to be reduced ſmall enough to go beyond the larger paſſages, and none of them are worth the pains they require to fit them farther than for diuretics; beſides, their natural fitneſs to attract and join with the ſerous part of the blood, whenever they get into that ſtage of motion, runs them off by the kidneys, before they can undergo comminution enough to get farther; but if by frequent repetitions of ſuch medicines and uncommon laxity of the paſſages, any thing be paſſed into the habit, their groſsneſs fouls the delicate ſtrainers, which are left for their expulſion; and they lodge upon the glands and capillaries in ſuch manner, as induces intermittents, which is obſervable in ſeveral patients after the uſe of cream of tartar, the common cathartic ſalts and the purging waters; eſpecially at the latter end of the ſummer, when the heat of the preceeding ſeaſon has debilitated the ſolids and left them under too great a relaxation.

Among the reſinous purges, there are ſeveral very powerful ones; but where their operation is propoſed in the *viſcera*, blood and remoter parts, they muſt be exceedingly divided; and this we find ſpirituous *menſtruums* will do by taking up the moſt ſubtile parts only, and conveying them into very ſmall paſſages, where their operation is chiefly by fuſion, becauſe the ſoftneſs of ſuch ſubſtances cannot make them, hardly in any degree, to act as *ſtimuli*, farther at leaſt than ordinary detergents: And thus we find aloes, the chief of this tribe, to go farther into the habit, and be longer before it operates, when managed in a ſpirituous *menſtruum*, as in the *Tinctura ſacra*; the *Rad. Turpeth*: And likewiſe *colocynth*, with all of the vegetable kind, that will yield to a ſpirituous liquor, are to be conveyed by that means into the fartheſt ſcenes of animal action; and there prove efficacious medicines in caſes, that with other management, they would never be able to reach: And on this foot it undoubtedly muſt have been,

been, that in practical writers we meet with many of this sort mentioned, as alterants; the *colocynth* particularly by *Helmont*; for, they commonly comprised under that general appellation all medicines that operated in the remotest passages.

But the most efficacious purges and those which require the most skill are from the mineral kingdom; these abound in solidity beyond any other material; and therefore, wheresoever they are brought into action, excel in quantity of impulse: Several of these, therefore, require not only the utmost comminution to convey them into the farther scenes of operation; but also some restraint to their asperities and motions, to fit them for several intentions: Thus sublimate is not only to be much sweetened, that is, smoothed in its points, to make it a safe purge in the larger vessels; but if it is intended to go farther than the blood, and those glands, which in that circuit, they are most apt to be lodged upon, when it salivates, it must be rendered not only very fine, but covered with such substances, as weaken its points and make it strain into the last subdivisions of the constitution. To this purpose, the common practice wisely contrives in distempers that lie fartheſt off, according to the course of circulation, to wrap up the basis of this medicine in sulphurs and such like substances, as follow it into its last division, without imparting to it any asperities to make it act as a *stimulus*. Thus for all cutaneous foulnesses and habitual taints, the cinnabar, the æthiops and all of that sortment are in readiness; and that ordinary sulphurs will cover and deaden the efficacies of mercurial preparations; so that they shall not operate, but in such parts only and in certain circumstances, is demonstrable in ordinary salivations, which are to be lowered at pleasure by sulphureous medicines.

Medicines from such minerals, where a salt and sulphur are united by nature; as they are in some mercurials by art, as antimony, native cinnabar, steel, *&c.* are manageable only upon the same principles; and the more they are designed to be carried into the habit, the more are they to be restrained by their natural or adventitious sulphurs: Steel, when opened by and united with the points of acid liquors, operates sooner, and will sometimes prove emetic; but when it is covered with an additional sulphur, it will go farther, and answer intentions much more remote, as is manifest in the common preparations of steel with tartar or vinegar, and with sulphur.

This way of thinking on these occasions seems the more just, from considering the texture of those substances, which by a na-

tural preparation are fitted for operation in the minuteſt part of an animal body; ſuch as thoſe of the aromatic kind; all which more or leſs, according to their greater or leſſer degree of ſubtilty and ſmoothneſs promote a *diaphoreſis:* They conſiſt of exquiſitely fine ſalts, covered with a very ſubtile ſulphur, as is demonſtrable by chemical analyſis; and the common *ſal volatile oleoſum* is an admirable contrivance upon the ſame foundation, where a very volatile animal ſalt is covered with a moſt exalted vegetable oil, whereby it is adapted to paſs into the minuteſt fibres, and make, as it were, a part of the animal ſpirits themſelves.

And here it may not be amiſs to obſerve, that all animal ſalts are very volatile, or are eaſily rendered ſo; but when bare and naked, juſt as extracted by the fire, with a mixture alſo of its own particles in their compoſition, they are too pungent to be felt without painful ſenſations; but when ſoftened with a fine portion of an oppoſite texture, which is ſmooth and yielding, they become moſt efficacious and ſafe ſudorifics.

On theſe conſiderations we are no longer ſurpriſed at the ſubtile ſalts of *cantharides* more ſenſibly affecting the bladder than any other parts, and why camphire prevents thoſe injuries; for, the exquiſite ſmallneſs of thoſe *ſpicula* makes them imperceptible, but in the moſt minute canals, into which the fibres that compoſe the membranes of the bladder are known to be divided; and camphire blunts their irritations, becauſe its exquiſite ſubtilty enables it to follow them into thoſe meanders, and ſheath their aſperities.

To this purpoſe it is very remarkable what ſeveral commonly practiſe in guarding even mercurials againſt their ſtimulating properties, and ſending them into the fineſt paſſages to operate by fuſion, and the bare force of impulſe; for, not only calomel and the *mercurius dulcis* may be reſtrained from manifeſt operation in the wider paſſages and the glands about the mouth; but even mineral turbith, which of itſelf in a ſmall doſe will operate powerfully by vomit and ſtool, when mixed with camphire, will not be ſo much felt in thoſe reſpects, but go into the fartheſt circuit of motion, and promote the cutaneous diſcharge in a more efficacious manner than any medicine of leſs ſpecific gravity; but in this management the camphire is to be mixed but very little before taking, otherways it hath not this effect, which appears to be from its great volatility, that makes it in a great meaſure exhale, while it ſtands mixed in a medicine.

This theory is applicable to ſeveral good purpoſes in practice; and this only inſtance of camphire is ſufficient to ſuggeſt to ſuch

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as turn their thoughts this way, in what caſes that and ſuch ſubſtances of like ſubtility and texture may be uſed with ſucceſs; for, the ſeat and cauſes of many chronic diſtempers lie very remote in the courſe of circulation, and the reaſon why they elude the ordinary means of cure, it is probable, is owing to the want of ſufficient attention to that particular management of efficacious remedies, which is neceſſary to carry their operations ſo far: An ordinary judgment knows how to intend or remit the efficacy of medicines, by ſharpening their points and increaſing their quantity of impulſe; or ſoftening and weakening them with broths and the groſs-expreſſed oils of almonds or linſeeds in the firſt and larger paſſages; but an active medicine, or a diſtempered irritating ſalt in the minuteſt capillaries and fibres, is not to be managed by ſuch coarſe methods.

An Account of an extraordinary Cramp *and* Fiſtula; *by Dr.* Steigerthal. Phil. Tranſ. N° 365. p. 79.

JOHN *Henry Oizmann*, aged 31 years and born at *Barum*, was 15 years old, when the following misfortune befel him.

He felt a *ſpaſmus* or cramp in his left hip and the lower part of his leg; as this pain ſeiſed him pretty often, he conſulted Mr. *Raek* a ſurgeon at *Ulzen*, who applied ſeveral plaiſters to the part affected; but without any relief to the patient: After all thoſe fruitleſs efforts, the ſurgeon made about 37 inciſions over the patient's whole leg, (which to outward appearance was become very brown) of which he was not at all ſenſible, unleſs when the inſtrument happened to grate upon the bone; the *perioſteum* being ſtill ſound: His leg however grew daily blacker, and the pain continued both in the *perioſteum* and in all the bones of the upper and lower part of the leg: At laſt a black circle was obſerved round about the muſcles of the hip, as an indication of an approaching mortification. This circle appeared ſo viſibly, as if it had been ſeparated with a knife from the other part. It ever after ſpread, and came to ſuch a head, that without any other help and cure, the fleſh began gradually to rot away from the bones; and at laſt quite fall off from the upper part of the leg, which preſerved its ſoundneſs; after this nothing was ſeen, but the bare tendons or ſinews hanging down like ſo many ſtrings or cords; there alſo remained a piece of the inferior muſcles of the hip faſtened to the ſuperior part. At laſt the tendons, becoming dry, conſumed away; and after all, the leg itſelf, to wit, the *os femoris* dropped off in ſuch a manner, that there remained about four inches between the bones and the fleſh, looſely hanging down

down from them. The fleſh at laſt grew up to the bones, without any manner of help, and faſtened itſelf to the bone; and in this ſound part the patient felt a great pain, whenever the weather proved tempeſtuous. It was remarkable, that at the ſame time the patient alſo perceived a ſwelling in the *tarſus* of the right foot, the matter of which diſcharged itſelf thro' the toes and was of ſo corroſive a nature, that it had conſumed all the toes but the little one. The ſurgeon at length healed up the wound; but after all, there was ſtill but little feeling or warmth in the foot.

This man was afterwards married to a woman 41 years old, whoſe bodily conſtitution was almoſt as remarkable: In her younger years ſhe was gored by a wild boar under the ſhort ribs of the left ſide; this wound became fiſtulous, and what food ſhe eat was diſcharged, half concocted thro' this aperture; ſo that you might diſtinguiſh what ſort of food it was; however, notwithſtanding this, ſhe had her daily evacuation *per anum*.

An Experiment to prove that there is an interſperſed Vacuum; *by Dr.* Deſaguliers. Phil. Tranſ. N°. 365. p. 81.

THAT bodies of the ſame bulk do not contain equal quantities of matter; and therefore that there is an interſperſed *vacuum* Dr. *Deſaguliers* proves by the following experiment.

He took three pounds of mercury, which filled thrice a ſmall glaſs jar exactly full, and he poured it into a thin *Florence* flaſk: Afterwards having poured the ſame quantity of water (that is three of the ſame jars full) into another ſuch flaſk, he ſet both the flaſks in a pail and pouring boiling water about them, keeping the flaſk that had the water forcibly down, that it might ſtand as low in the hot water as the mercury: After the fluids in the flaſks had received a ſufficient degree of heat from the water, for the ſpace of five minutes; he took the flaſks out of the hot water, and putting that which held the water into a cylindric veſſel that had three pints of cold water in it, he at the ſame time plunged the flaſk with mercury into another cylindric veſſel, that alſo contained three pints of cold water; and he obſerved which of the cold waters was moſt heated in the following manner.

Upon holding a little thermometer in the firſt veſſel of cold water, ſo as to have its ball covered with the water; when the flaſk of warm water was put in, the ſpirits roſe two degrees;

grees; then putting the thermometer into the water where the flask of mercury stood, the spirits rose three degrees higher: The thermometer being again put into the first vessel, they fell four degrees; and afterwards again into the last, they rose almost three degrees.

This shews, that more heat is communicated by warm mercury than by an equal bulk of water equally heated; and therefore, that there is more matter in the mercury; but how much more, is not determined by this experiment alone.

N. B. The warm mercury and warm water were not poured into the cold; but only communicated their heat thro' the flasks.

An Account of a Contagion *among the* Cattle *in the* Venetian Territories *in Autumn* 1711; *by Dr.* Michelotti. Phil. Trans. N° 365. p. 83.

DR. *Michelotti*, being in the *Venetian* territories about *October* 1711, took that opportunity of making a particular enquiry into the circumstances of the mortality that reigned at that time among the black cattle; and was himself an eye-witness of the greatest part of the facts contained in this account, and received the rest upon the spot from persons of integrity and credit.

Almost all the sick cattle refused every sort of food and drink; they hung their heads, had shiverings in their skin and in their limbs; they breathed with difficulty and their expiration in particular was attended with a sort of rattling noise; they were so feeble, that they could hardly go or stand upon their legs: Some few of them eat a little and drank very much; others had fluxes of excrement variously coloured, of a very offensive smell and frequently tinged with blood: Several of them had their heads and bellies swelled in such a manner, that upon clapping them with the hand on their paunches, or along the *vertebræ* of the loins, they sounded like a dry bladder, when full blown: In some the urine was very turbid, in others of a bright flame-colour. In comparing the pulses of the sound cattle with those of the diseased, the Dr. found the latter to be quicker and weaker: There was but little heat perceivable by the touch in any of them; their tongues were soft and moist, but their breath exceedingly offensive: Besides these particulars, he was informed by those who attended the sick cattle and other persons of credit, that in some of them they observed crude tumours in several parts of the body,

body, as alſo watery puſtules and diſorderly motions of the head, with dry, black and fiſſured tongues, that in others of them they met with tumours that came to maturation; with putrid matter iſſuing from the mouth and noſtrils; worms in the *fæces* and in the eyes; bloody ſweats and ſhedding of the hair.

In comparing the fleſh of the cattle, that died of this diſtemper, with that of others killed for the market, the Dr. found the muſcles in the former, which lay immediately under the ſkin, to be ſomething livid: Having opened the three cavities of the body, he applied himſelf with the utmoſt diligence to examine the brain with its membranes, the *trachæa*, *oeſophagus* lungs, and heart with its auricles, the *vena cava*, *aorta*, the diaphragm, the liver, ſpleen and other parts of the lower belly; and in all theſe there was no diſcernible difference, either as to figure, ſize, contents, ſituation or connection with the neighbouring parts, from what was obſerved in ſound cattle killed by the butchers, excepting the particulars hereafter mentioned. The blood found in the ventricles of the heart, the pulmonary veſſels, the *aorta* and *cava* tho' ſtill warm, was conſiderably blackiſh and almoſt coagulated. In opening the upper and middle cavity, the ſcent was offenſive, but tolerable; whereas that, proceeding from the lower belly, was quite intolerable. In ſome few carcaſes the *viſcera* differed from what they are in a natural ſtate, with regard to their ſize, conſiſtence, contents, colour and ſmell: In ſeveral the paunch was found very much contracted and dried, with a hard ſubſtance contained therein: In others the lungs were ſwelled and livid, the liver tumefied, and the brain watery and putrid.

The Dr. having ordered ſeveral of the ſick cattle to be blooded, found, that the blood did not iſſue out of the veſſels in a continued ſtream, as uſual, but with a broken and interrupted flux, one part thereof not immediately ſucceeding another. Having cauſed the blood to be received in proper veſſels, and let it ſtand for ſome time, he found it entirely coagulated, without any ſeparation of the *ſerum*, and attached to the ſides of the veſſels, with a reticular pellicle upon the ſurface, expoſed to the air. All the cattle that were blooded, being 18 in number, died in a few days after the bleeding, excepting one only, in which the vein was opened upon its being firſt taken ill.

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Having enumerated all the ſymptoms of the diſeaſe; the Dr. concludes from the whole, that this ſickneſs among the cattle was a malignant peſtilential fever, killing almoſt all thoſe that were infected with it.

The immediate cauſe of this he takes to be a preternatural thickneſs of the blood, occaſioned by a coagulation beginning in thoſe parts of it which conſtitute the *craſſamentum*; whereby the globules of the blood and the particles of the *ſerum* were impriſoned in a ſort of *reticulum*, formed by the union of the fibres of the blood.

The occaſional cauſe of this ſickneſs he deduces from the cold and wetneſs of the ſeaſon, that prevailed all the preceeding year from *October* 1710 to *November* 1711; which obſervation is worthy of remark, ſince the ſeaſon, preceeding the mortality among the cattle in *England*, was remarkably dry, and yet the ſymptoms of the diſtemper agreed with thoſe obſerved in *Italy*, as appears from Mr. *Bates*'s account in *Phil. Tranſ.* N° 358.

A Portion of the Colon *hanging out thro' a Wound for* 14 *Years; by Dr.* Abraham Vater. Phil. Tranſ. N° 366. p. 89. *Tranſlated from the* Latin.

ONE *George Deppe*, a native of *Halberſtadt*, and 34 years of age, by a wound in the left *hypochondrium* in the battle of *Ramillies* in 1706, had for 14 years a large portion of the *colon*, by this means cut and inverted, hanging out of his body, a ſpan in length.

This inteſtine coming out thro' the wound, that at the ſame time penetrated into it was turned inſide out; and cohering thus in the middle formed two portions; one of which extending upwards had an orifice, regarding the thin guts, and diſcharging the *fæces*; the other, hanging downwards had an orifice opening into the *rectum*, ſo as to diſcharge by the *anus* a glyſter injected into it: On its inner ſurface, now turned outwards, a great number of glands, of a white and cineritious colour appeared, prominent like warts, and affording a very agreeable ſight; theſe, when roughly handled, yielded blood: The inteſtine thus hanging out was never wholly retracted into the *abdomen*, tho' it was a little retracted, when the ſtomach happened to be empty; but when full, it was farther protruded, eſpecially, on retaining his breath. He uſed without any inconveniency the coldeſt water, nay that mixed with ice and ſnow, to waſh off the filth; and the inteſtine could

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bear the coldeſt air, only that it made it ſhrink, grow hard and ſomewhat pale: Any ſort of food agreed with him; yet new fruit and greens did not incorporate with his other victuals, but were diſcharged undigeſted; as alſo broths taken without ſolid food.

A B C D Fig. 1. Plate III. repreſents the *colon* turned inſide out, hanging out at the wound, and reaching upwards and downwards; *a* the ſuperior orifice opening into the thin guts, thro' which the *fæces* were diſcharged; *b* the inferior orifice, leading to the *rectum*, thro' which a glyſter was diſcharged by the *anus*; *c d* the veſtiges of the wound enlarged by the ſurgeon, thro' whoſe middle the inteſtine hung out; *e* the navel; *fff* great numbers of glandules; *g* the inguinal region; *h h* the *dorſum*; *i* the *coxendix*; K the lumbar region.

Obſervations on the Bones *and* Perioſteum; *by M.* Leewenhoeck. Phil. Tranſ. N° 366. p. 91.

M. *Leewenhoeck* found by frequent obſervations on bones, that their ſuperficial part conſiſts of a vaſt many ſmall veſſels, and ſome few of a larger ſize; which laſt, when they came to the ſurface of the bone, appeared to him inveſted either with a membrane or bony ſubſtance, perfectly tranſparent.

M. *Leewenhoeck* once diſcovered in a ſmall portion of a ſhin-bone four or five veſſels of ſuch a ſize, that a ſingle filament of ſilk might have been drawn thro' their aperture: One of theſe appeared to him to conſiſt of two openings; each of which ſeemed to be furniſhed with a valve, diſpoſed in ſuch a manner, as to let out what was contained in the veſſel, but ſuffer nothing to enter it.

As to the matter that iſſues out of the bones and is convey'd into the *perioſteum*, M. *Leewenhoeck* diſcovered its ſource to be in the ſpongy or cellular ſubſtance on the inſide of the bone; which is the repoſitory of the marrow.

This ſpongy ſubſtance conſiſts of long particles cloſely united and linked together, which are compoſed of a vaſt many ſmall veſſels, ſome running lengthways, and others taking their courſe towards the ſides of the bony particles; which laſt, notwithſtanding their great number of apertures, are yet exceeding hard, ſome of them lying parallel and others perpendicular to the length of the bone.

Thoſe particles, that lie perpendicular to the length of the bone, have veſſels proceeding from their extremities; and other veſſels that compoſe the *cortex* or ſuperficial part of the bone

bone proceed from their ſides, where they do not lie cloſe together; and thoſe long particles, that lie parallel to the length of the bone, emit veſſels from their ſides, that iſſue out thro' the ſide of the bone: It is impoſſible to conceive the prodigious number of ſmall veſſels of which the cortical part of the bone conſiſts, which in ſome places lies no thicker on the ſpongy part of the bone than a thick hair of a man's head; tho' in other places it has three or four times that thickneſs.

The *perioſteum* is united to the *cortex* of the bone, not only on the outſide, but even by entering in ſeveral places into its very ſubſtance, and is joined thereto by the veſſels, which iſſue out from the bone, in ſuch a manner, that ſometime one cannot determine which is the bone, and which belongs to the membrane inveſting it, they both appearing in the microſcope to conſiſt alike of exceeding ſmall veſſels.

Fig. 2. Plate III. repreſents a ſmall part of the bone with the *perioſteum* adhering thereto; A B C D E F the bony part; B G H I E the *perioſteum*, whoſe thickneſs is repreſented by B G or I E; tho' in other places of the bone, and even at no greater diſtance than two or three hairs breadth, it is twice or thrice as thick; all the ſmall veſſels in the *perioſteum* are repreſented by ſo many dots or points; but in other places, where M. *Leewenhoeck* had obſerved the membrane of twice this thickneſs, its upper part has appeared of a different make from the under; foraſmuch as he could diſcover in the upper part not only thoſe veſſels that had been cut tranſverſely, and conſequently, repreſented by ſo many points; but likewiſe a great number of other veſſels running lengthways along the membrane, as repreſented by L O P Q N M in Fig. 3.

M. *Leewenhoeck* is fully perſuaded, that the part repreſented by B G H I E in Fig. 2. is not entirely membranous, but that ſome part of it is really bony. Upon cutting thro' the *perioſteum* ſo deep, as to divide the part of the bone, marked A B C D E F in the ſame Fig. we find the ſame appearance of pores in the bony ſubſtance, which are no other than the tranſverſe ſections of ſmall veſſels; and beſides theſe, there are other veſſels running lengthways in the bone; and we obſerve juſt the ſame in thoſe tranſparent parts, that lie between the bony particles, which are repreſented thicker between B C D E than they appeared to M. *Leewenhoeck*.

M. *Leewenhoeck* ſuppoſes, that the uſe of theſe bony particles is to convey an oleaginous liquor into the *perioſteum*, and from thence by the intervention of the other membranes into all parts of the body, in a healthful ſtate.

In another place he obſerv'd a great number of veſſels ariſing from a greater depth within the bone, which drew cloſer together, ſo as to compoſe ſmall *faſciculi*, before they enter'd the *perioſteum*, in which they ſeparated from each other and diſpers'd themſelves again: It is hard to determine, whether theſe veſſels convey any liquor into, or from the bone; tho' M. *Leewenhoeck* rather thinks they ſerve for the latter purpoſe.

Having placed before the microſcope another piece of bone with the *perioſteum* adhering thereto, he could diſcover a great number of veſſels, cut thro' lengthways, as they ran along the *perioſteum*; and others cut thro' tranſverſely, appearing like ſo many points, as repreſented by KLOPQNA in Fig. 3. where the bony part is mark'd KLMNA, in which tho' no pores or veſſels are here repreſented; yet it is full of openings: That part repreſented by LOPQNM is not entirely membranous; for, M. *Leewenhoeck* ſuppoſes, that that part of it, which lies next the bone, and is repreſented by LMN, is of a bony ſubſtance.

He had another ſmall piece of bone, lying before a microſcope, part of which he caus'd to be repreſented by RSWXTV Fig. 4. RSTV is the bone; SWXT the *perioſteum*, which in this place was no thicker than a thick hair of a man's beard; but in another part of the ſame bone at a ſmall diſtance it was full four times that thickneſs.

M. *Leewenhoeck* placed before a microſcope another piece of bone in ſuch a manner, as that the bone did not appear; but only the *perioſteum* and muſcular fibres, which were cut thro' tranſverſely and appeared to be ſurrounded by the fibrils of the *perioſteum*, as repreſented by YZCDAB in Fig. 5. where YZAB is the *perioſteum* and ZCDA are the fleſhy fibres cut thro' tranſverſely: This piece of bone was taken from one of the ribs of a fat ox; and he was ſurpriz'd, that in this place, as he cut thro' the rib lengthways, he could not diſcover any particles of the marrow; whereas in other parts the rib abounded with them.

Notwithſtanding the great number of obſervations M. *Leewenhoeck* made on the bones, and the membrane inveſting them, *viz.* the *perioſteum*, he could not ſatisfy himſelf entirely about them: He ſtill imagin'd, that the part of the *perioſteum*, which immediately covers the bone and is cloſely united thereto, muſt have a degree of hardneſs approaching to that of the bone; and that at a ſmall diſtance from the bone, the

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perioſteum muſt have a ſoftneſs and flexibility like that of the carneous and adipoſe membranes.

M. *Leewenhoeck* had kept four pieces of the ribs of a fat ox full two months, which were then become very dry: From one of theſe he tore off the *perioſteum*, which he found ſtick harder to the bone than he could have imagin'd; and he obſerv'd that a great many particles of this membrane ſtill adher'd to the bone. He did this with a deſign to make ſome obſervations on the ſuperficial part of the bone, which is not near ſo hard, as thoſe bony particles that lie a little deeper: From this bone he cut off ſome very thin ſlices, both lengthways and tranſverſely; one of which he placed before the microſcope and caus'd it to be delineated.

This piece is repreſented by ABKC Fig. 6. having been cut off tranſverſely, and as thin as poſſible, from the rib together with part of the *perioſteum*, as from K to C, ſtill adhering to the bone, and another part torn off from the bone, as repreſented by BKD, only that in ſome places the bone and membrane are ſtill united by veſſels torn out of their places, and that run from one to the other.

In this figure DEFC repreſents the *perioſteum*; the part deſign'd by EGHIF in ſome meaſure lies upon it, which M. *Leewenhoeck* did not know what to make of, tho' it appear'd to him to be membranous.

He had likewiſe ſome very thin ſlices ſhaven off from the rib both of an ox and a calf, from which he tore off the *perioſteum* entirely, or at leaſt as much as poſſible; afterwards he caus'd the edge of the bone, it had ſtuck to, to be repreſented by the crooked line LMN Fig. 7.

OPQ Fig. 8. repreſents the edge of another ſmall ſlice of bone, from which the *perioſteum* had been torn off; by which appearance it ſhould ſeem, that the union of the *perioſteum* with the bone is ſo firm and ſtrong, that in ſeparating it, ſome of the ſuperficial particles of the bone are torn off with it.

He likewiſe diſcover'd ſome veſſels running along within the marrow bone of the ſhank of an ox, that ſeemed to be blood-veſſels.

Since it appears from M. *Leewenhoeck*'s obſervations, made with the utmoſt diligence and care on bones of all kinds, that they do for the moſt part conſiſt of exceeding ſmall veſſels, which ariſe from the inner, hollow or ſpongy part of the bone, and paſſing thro' the ſuperficial or cortical ſubſtance, enter the *perioſteum*; and are from thence continued farther into the

body, yea even into the utmoſt parts thereof; we may hence reaſonably conclude, that in a healthful body, as there is a conſtant ſupply of an oily ſubſtance convey'd into the bones; ſo this is again conſtantly carried out from the bones, by means of theſe veſſels, into all parts of the body, even to the extremities of the fingers: As an evident proof of this, let any one lay the ends of his fingers upon a clean and bright pewter plate, and he ſhall find the pewter appear ſoil'd in the place where he has touch'd it; for, in reality this ſoil is nothing elſe but ſome oleaginous particles diſcharged from the ends of the fingers; there is, it is true, ſomething of a watery ſubſtance mix'd with the oily particles; but this evaporates in a little time, and the oily particles are left behind upon the plate.

A preternatural Tumour on the Loins of an Infant attended with a cloven Spine; *by Dr.* Rutty. Phil. Tranſ. N° 366. p. 98.

THE accurate M. *Ruyſch*, in an obſervation on the *ſpina biſida*, takes notice, that other writers have deſcrib'd it, as cloven lengthways into two equal parts; whereas out of 10 ſubjects he had an opportunity of examining, not one of them prov'd in that manner, the body of the *vertebræ* being entire and the accute proceſſes only divided.

The ſpine Dr. *Rutty* treats of comes near this deſcription: It was that of a female infant ſix days old; whoſe mother, when ſeven weeks gone with child, upon a fright occaſion'd by her husbands falling from a horſe and very much bruiſing his loins, gave the embryo this injury; but notwithſtanding, ſhe went out her time, and the child was full grown.

There appear'd upon the region of the loins, in the ſame part where the father receiv'd his hurt, a tumour about the bigneſs of a ſmall turnep, with a broad baſis, around which the ſkin was diſcoloured by an *ecchymoſis*, as it were; but from thence it immediately became pellucid, reſembling a bliſter rais'd by *cantharides*, and continued ſo throughout, excepting juſt at its *apex*, where was a ſubſtance like a *fungus*: This veſicle was fill'd with a liquor, which in ſcent and colour reſembled urine; inſomuch, that upon ſtrictly examining the linen ſtain'd by what iſſu'd from it, with that from the *urethra*, no ſenſible difference could be perceiv'd, and a communication was concluded between the left kidney and the orifice, into which the

the ſurgeon's probe paſs'd obliquely upwards about an inch: Whether the ſame ſimilitude may hold between the urine and this liquor in all caſes of the like nature, Dr. *Rutty* has not had opportunity of obſerving; but as M. *Ruyſch* has taken no notice of it in ſuch a number of ſubjects that have been before him, he is apt to think that it does not.

Upon opening the body, the kidneys, contrary to expectation, were entire, and did not any way communicate with the outward orifice.

But upon clearing away the fungous ſubſtance, which took up all the *ſulcus* or hollow of the ſpine, then it was diſcover'd where the perforation tended, a long probe eaſily paſſing up the channel, which contains the ſpinal marrow: Throughout this *fungus* were diſpers'd a great many terminations of ſmall nerves, from whence diſtill'd this urinous liquor, as it were, which occaſioned the tumour: The reſt of the *medulla* was more compact and fill'd the cavity of the ſpine; tho' in ſome ſubjects it has been waſted to ſuch a degree, that by blowing into the orifice you might inflate the *dura mater*; as was done in a like caſe by Mr. *Dobyns* a ſurgeon. The coat of the tumour at its baſis conſiſted of the *cuticula*, *cutis* and *membrana adipoſa*; the two firſt of which terminating forthwith, the *cuticula* only was continued, under which immediately appear'd the muſcles and fungus above mentioned: In this it differs from thoſe of M. *Ruyſch*, which receiv'd their coats from the membranes that inveſt the *medulla*; as alſo from thoſe taken notice of by *Tulpius*, that borrow'd theirs from the *peritonæum*, as he ſays; tho' by his own deſcription, it is more likely they proceeded from the ſame membranes.

Theſe tumours conſtantly attend the *ſpina bifida*; inſomuch that when any of them preſent themſelves on the loins or back of a new born infant, we have good grounds to pronounce this to be the cauſe; but we may be poſitive therein, if the child cannot move its lower limbs, the want of ſuch motion is an infallible indication, that the ſpinal marrow reaches no farther than the ſwelling, whereby the nerves are not diſtributed to thoſe parts: They appear differently in different ſubjects; in ſome the whole tumour is opaque, which is owing to ſmall filaments of nerves, propagated in great numbers throughout its coat, and not from the thickneſs of the ſkin, as M. *Ruyſch* would have it; in others it is pellucid, and then the *medulla* terminates at once at the aperture of the ſpine, and does not ſhoot out into any ramifications: This before us is a compound

of

of both ſpecies, the greater part of which was pellucid; the leſs, *viz.* the *apex* opaque.

The ſpine itſelf was not cleft; but had its *vertebræ*, with their other proceſſes entire, and was only defective in its acute ones: But that portion of the *vertebræ*, that ſhould form an acute angle, from whence their ſpinal proceſſes naturally ariſe, in order to form the *ſpecus* ot paſſage for the marrow, gap'd and lay almoſt in a ſtreight line on each ſide; whereby the *medulla* was deprived of its uſual defence from external injuries. This defect began at the third *vertebra* of the loins, and was continued to the end of the *Os ſacrum*; which laſt is very extraordinary, it ſcarcely ever happening to this bone; the reaſon of which the Dr. takes to be the more than ordinary compactneſs of its *vertebræ*, whereby it is leſs liable to be injured by any impreſſion: For, we obſerve that in adults they grow ſo cloſe together, as to unite and form one large and ſolid bone.

As the caſe before us is a *vitium conformationis*, owing to the mother's imagination; ſo the ſame is ſometimes occaſioned by matter lodging upon the ſpine and corroding the *vertebræ* by its acrimony; but then you find a carious bone; whereas, in theſe preternatural caſes, there remain no ſuch veſtiges. This was obſerv'd by Mr. *Cowper* in the body of the late Earl of *Peterborough*; for, upon opening it, he found a large tumour, from which there flow'd a browniſh colour'd matter; and under it the upper and fore-part of the ninth and the lower part of the eighth *vertebra* of the *thorax* were conſum'd and gone, the ſpinal marrow being cover'd with its membranes only: This gave riſe to thoſe ſymptoms in that morbid caſe, ſo nearly reſembling the above recited in this preternatural one.

It is manifeſt from what has been ſaid, that theſe caſes are incurable, and muſt in a little time kill the patient: But it is almoſt immediate death to open the tumour, which every ſurgeon will naturally do, the fluid ſeeming to require a diſcharge.

Of the change of Colour in Grapes *and* Jeſſamine; *by Mr.* Cane. Phil. Tranſ. N° 366. p. 102.

IN 1714 Mr. *Cane* planted a cutting of a vine againſt a wall, on an eaſtern proſpect; where it had the ſun from his riſing till half an hour after 12 o'clock: The ſoil was a ſtiff clay, but to make it work the better, he meliorated it by mixing ſome

ſome rubbiſh of the foundation of an old brick wall; in *January* 1719 he prun'd it and its figure was thus,

Left hand T right hand *black*.

At time of year it ſhot at both hands about 22 inches of a ſide, before it came to a joint; that on the right was a very luxuriant branch, as big as the body of the tree; the other ſide was not half ſo thick or big; and the leaves on the right were as big again as the other on the left hand, and the largeſt he thinks he ever ſaw: The right hand bore a very large and good black grape and large cluſters; the left hand were very good white grapes; and in 1719 he had more bunches of the white than of the black; and whereas in all vines bearing black and blue grapes the leaves die red, theſe died white on the black ſide as well as on the other: In *January* 1720 he prun'd the tree again, but tack'd up more of the right hand (being black) than he did on the left; for which reaſon he had that year a great many more of the black than he had of the white; and they ripen'd for the ſeaſon of the year very well; about the 23d of *October* 1720, he gather'd the laſt of them; and the leaves died alſo white this year, being the ſecond year of its bearing.

In *April* 1692 Mr. *Cane* having a ſmall plant of the common white jeſſamine, which ſtood in the ground, and was no bigger than a tobacco-pipe; he cut it off at two joints above the ground, and grafted it with a cutting of the yellow-ſtrip'd; it took and ſhot a ſmall weak ſhoot; and in a month or five weeks after, it was blighted; and he perceiv'd it had kill'd the graft and ſome part of the ſtock below; ſo he took his knife and cut it to the quick, which was near the knot or joint next the ground, and let it ſtand, thinking to graft it again in ſpring, as before, but he forgot it till the ſeaſon was paſs'd: At length he obſerv'd, that it had broke out at the next joint with ſeveral ſhoots of the yellow and green ſtrip'd; and not only there, but alſo from the root it had a ſtrong ſhoot of yellow and green ſtrip'd: Some time after he took it up with mold to the root; and putting it into a pot it flouriſh'd all the ſummer: He made a preſent of it to the Preſident of *Magdalen* College *Oxon*, where it flouriſh'd two or three years; and then for want of ſhifting the pot in time, it was ſo matted to the bottom and ſides thereof, that it died.

Mr.

Mr. *Cane* tried ſeveral other ſorts of variegated plants; but he did not find any of them to tranſmute, as that jeſſamine will do.

An Account of ſome new electrical Experiments; *by Mr.* Stephen Gray. Phil. Tranſ. N° 366. p. 104.

MR. *Gray* having often obſerved, in the electrical experiments made with a glaſs tube and a down feather, tied to the end of a ſmall ſtick, that after its fibres had been attracted towards the tube, when that has been withdrawn, moſt of them would be attracted to the ſtick, as if it had been an electrical body, or as if there had been ſome electricity communicated to the ſtick or feather: This put Mr. *Gray* upon thinking, whether if a feather were drawn thro' his fingers, it might not produce the ſame effect, by acquiring ſome degree of electricity: This ſucceeded accordingly upon his firſt trial; the ſmall downy fibres of the feather next the quill being attracted by his fingers when held near it; and ſometimes the upper part of the feather with its ſtem would likewiſe be attracted; but not always with the ſame ſucceſs. He then tried, whether hair might not have the ſame property by taking one from his peruke, and drawing it three or four times thro' his fingers, or rather between his thumb and fore-finger; and he ſoon found that at the diſtance of ½ an inch it would be attracted to his finger; and ſoon after he found, that the fine hair of a dogs ear was ſtrongly electrical; for, upon taking the ear and drawing it thro' his fingers, great numbers of them would be attracted towards his fingers at once.

The next thing Mr. *Gray* thought of, was threads of ſilk of ſeveral colours and ſeveral fineneſſes, which he found to be all electrical; but ſometimes he could not ſucceed: The reaſon of which he afterwards found, as will appear anon.

Having ſucceeded ſo well in theſe, Mr. *Gray* proceeded to larger quantities of the ſame materials; as pieces of ribband both of coarſe and fine ſilk, and of ſeveral colours; and he found, that by taking a piece of either of theſe of about half a yard long, and by holding the end in one hand and drawing it thro' his other hand between his thumb and fingers, it would acquire an electricity; ſo that if the hand were held near the lower end of it, it would be attracted by it at the diſtance of five or ſix inches; but at ſome times the electricity would be much weaker than at others; the reaſon of which he conjectured to be, that the ribband might have imbibed ſome aqueous particles from the moiſt air, which upon trial he found to be the caſe;

caſe; for, when he had well warmed the ribband by the fire, it never failed to be ſtrongly electrical.

After this he made trial with ſeveral other bodies, as linen of ſeveral ſorts, *viz.* holland, muſlin, *&c.* and woollen, as of ſeveral ſorts of cloth and other ſtuffs of the ſame materials: From theſe he proceeded to paper, both white and brown; and he found, that, after they had been well heated before rubbing, they copiouſly emitted their electric effluvia: The next body, in which he found the ſame property, was thin ſhavings of wood; he hitherto only tried fir-ſhavings, which are ſtrongly electrical: The three laſt ſubſtances he found to have the ſame property are leather, parchment, and thoſe thin guts wherein leaf-gold is beaten.

All theſe bodies will not only by their electricity be attracted to the hand or any other ſolid body near them; but they will, as other electrical bodies do, attract all ſmall bodies to them, and that at the diſtance of ſometimes 8 or 10 inches: Heating them at the fire before rubbing very much increaſes their force.

There is another property in ſome of theſe bodies, that is common to glaſs, *viz.* that when they are rubbed in the dark, there is a light follows the fingers thro' which they are drawn; this holds both in ſilk and linen, but is ſtrongeſt in pieces of white preſſing papers, which are much the ſame with card-paper; this not only yields a light as above, but when the fingers are held near it, there proceeds a light from them with a crackling noiſe like that produced by a glaſs-tube, tho' not at ſo great a diſtance from the fingers; to perform this, the paper, before rubbing, muſt be heated as hot as the fingers can well bear.

A down-feather being tied to the end of a fine thread of raw ſilk, and the other end to a ſmall ſtick, which was fixed to a foot, that it might ſtand upright on the table; there was taken a piece of brown paper, which by the above-mentioned method, was made to be ſtrongly electrical; and which, being held near the feather, attracted it; and with the ſame he carried it, till it came near the perpendicular of the ſtick; then lifting up his hand till the paper was got beyond the feather, the thread was extended and ſtood upright in the air, as if it had been a piece of wire, tho' the feather was diſtant from the paper near an inch: If the finger were held near the feather in this poſition, the greateſt part of the fibres next the paper would be repelled; when at the ſame time upon holding a finger to the fibres that were more remote from the paper, they would be attracted thereby.

Mr. *Gray* repeated this experiment without the feather, *viz* by a ſingle thread of ſilk only about five or ſix inches long, which was made to ſtand extended upright as above-mentioned, without touching the paper; then placing his finger near the end, it would avoid or was repelled by it; but upon placing his finger at about the ſame diſtance from a part of the thread, that was about two inches from the end, it was then attracted by it.

The ſeveral bodies found to be electrical are, 1. Feathers. 2. Hair. 3. Silk. 4. Linen. 5. Woollen. 6. Paper. 7. Leather. 8. Wood. 9. Parchment. 10. Ox-guts, in which leaf-gold is beaten.

Some Objections, made to the Account of the Antiquity of the Venereal Diſeaſe, *anſwered; by Mr.* Becket. Phil. Tranſ. N° 366. p. 108.

THE two following objections were made to Mr. *Becket's* account of the antiquity of the venereal diſeaſe in *Phil. Tranſ.* N° 357, 365. The firſt is, that the venereal diſeaſe, ſo well known among us now, and the leproſy of former ages, could not be the ſame diſeaſe; becauſe the leproſy is not to be conquered by ſalivation, to which the other generally yields very readily.

In anſwer to this Mr. *Becket* obſerves, that the leproſy we have among us at this time, only affects the ſurface of the body; the ſkin generally appears ſcaley, with a certain deep red colour or ſmall ſores upon removing the ſcales, and ſometimes a ſcabbineſs, with a redneſs of the ſkin that affects different parts of the body. Mr. *Becket* has known both the cheeks only affected; both the arms for the breadth of the palm of the hand; ſometimes the breaſt, the legs and other parts; but this may continue upon the patient during his life, as it frequently does, and never make any farther progreſs; which ſhews it to be a cuticular diſeaſe. In theſe caſes upon ſalivating the patients, the ſcales generally fall off, the redneſs diſappears, and the cure ſhall ſeem to be compleated; but in a month or two, the ſame inconveniencies generally attend them as before. But one ought not to conclude, that becauſe our leproſy will but rarely be cured by ſalivation, and the pox generally will, that many of thoſe the ancients judged to be leprous were not really venereal; for, their leproſy, as they called it, was a quite different diſeaſe from ours: Had there been any proof adduced, that perſons had been ſalivated in their leproſy and failed of cure, it would have determined the caſe. But on the contrary we are aſſured by the learned

learned Dr. *Pitcairn*, in his deſertation on the ingreſs of the *lues venerea*, that the leproſy, before the *Neapolitan* diſeaſe was talked of, was cured by mercury; and now ſince it changed its name, it is no longer heard of. Thus we find, that their leproſy and our venereal diſeaſe would be cured by the ſame method; but their leproſy and ours being abſolutely different diſeaſes, we by no means ought to expect the ſucceſs, from the ſame proceſs of cure, ſhould be the ſame. None ever obſerved our leproſy to be attended with ſhedding of the hair, hoarſeneſs of the voice, ſpeaking thro' the noſe, conſumption of the fleſh, ulcers all over the body, putrefaction of the fleſhy parts and of the bones themſelves, filthy ulcers of the throat, corroſion and falling of the noſe; all which are reckoned as ſymptoms of their leproſy: On the contrary, ours is a mild and almoſt inoffenſive diſeaſe, which a perſon may be affected with all his life long, and never become the worſe: Whereas the other ſhewing itſelf by the above-mentioned ſymptoms, brings the patient to the moſt miſerable end: Beſides, their diſeaſe was got by coition, as their authors aſſures us; but in our leproſy, a diſeaſed husband may cohabit with his wife, as long as he lives, and he ſhall never be able, either by coition or the immediate contact of the diſeaſed parts with thoſe that are ſound, to communicate any malady. Had what our predeceſſors called the leproſy been the ſame diſeaſe we call by that name now, they had not been ſo ſolicitous of making ſuch large proviſion for them or ſhutting them up from human ſociety; for, one of our leprous perſons might have been among them and no body have known that he laboured under any infirmity at all.

Hence it is evident, that the diſeaſe, ſo common among them, was entirely different from our leproſy, the appearances of which bear no manner of analogy with the former. It is from the ſymptoms of the diſeaſe and the manner of its being received, that we generally diſtinguiſh one diſeaſe from another; but the ſymptoms of moſt of their leprous patients and the manner in which the diſeaſe was gotten, will be found in no other diſeaſe, that attacks the human body, but in the venereal only; for, here they exactly agree; and conſequently, we cannot deny them to be the ſame.

The ſecond objection is a falſe aſſertion of Dr. *Fuller*'s the hiſtorian; *viz.* that the leproſy was brought into *England* by ſome of our countrymen from the holy war, and that the diſeaſe was altogether unknown among us before.

In anſwer to this objection, Mr. *Becket* obſerves, that it was in 1096 the firſt *Engliſhmen* went over to the holy war, as our hiſtorians generally agree; and that ſome of them returned in 1098, two years after that expedition: But it is very certain, we had the leproſy among us before that time; for, *Wharton de Epiſcopis Londinenſibus* and other hiſtorians aſſure us, that *Hugo de Orivalle*, one of the Biſhops of *London*, died here of the leproſy in the year 1084; which proves, that our countrymen did not firſt bring that diſeaſe from the holy war: *William* of *Malmesbury*'s account of this diſeaſe is, as follows; *Is poſt paucos ordinationis annos in morbum incurabilem incidit; ſiquidem regia valetudo totum corpus ejus purulentis ulceribus occupans ad pudendum remedium tranſmiſit; nam credens aſſerentibus unicam fore ſubſidium, ſi vaſa humorum receptacula, verenda ſcilicet, exſecenter, non abnuit: Itaque & opprobrium ſpadonis tulit Epiſcopus, & nullum invenit remedium, quoad vixit Leproſus.*

Now it is highly probable, had this been a new diſeaſe the Biſhop died of, the mention of it, as ſuch, would not have eſcaped our hiſtorian: But on the contrary, it ſeems to have been anciently known among us, becauſe the remedy made uſe of for it was ſo; it having been recommended by *Ætius* and other phyſical writers ſeveral hundred years before this time; and Mr. *Becket* thinks it very plain, that the cutting off the *teſtes* and with them the veſſels deſigned for receiving the humours, as expreſſed in the former caſe, was by them looked upon to be of peculiar ſervice; becauſe it is probable, that obſerving the diſeaſe to begin in theſe and the neighbouring parts, they ſuppoſed that the very *minera morbi* would by this means be deſtroyed, and the diſeaſe either cured or its ſpreading prevented.

An Experiment to compare the Paris Weights *with the* Engliſh; *by Dr.* Deſaguliers. Phil. Tranſ. N° 366. p. 112.

DR. *Deſaguliers*, finding the accounts we have of the *French* weights different in different books, ſent for ſome *Paris* weights exactly according to the ſtandards at the *Chatelet*; and he found upon trial, the *Paris* ounce, which contains 576 of their grains, to be equal to 476 of our grains *Troy*; from which experiment all the other proportions may be deduced.

The *French* pound contains 16 ounces

Ounce	8 drachms or 576 *Paris* grains	
Drachm	3 deniers	
Denier	24 grains	

m Some

Some Remarks on the Method of obſerving the differences of right Aſcenſion and Declination by croſs Hairs in a Teleſcope; by Dr. Halley. Phil. Tranſ. N° 366. p. 113.

ASTronomers are greatly endebted to M. *Caſſini* for his invention of applying threads at half right angles in the common *focus* of a teleſcope, to determine thereby the differences of right aſcenſion and declination of any two ſtars, whoſe ſituation is ſuch, that by their diurnal motion they follow each other thro' the aperture of the teleſcope, fixed in ſuch a manner, as that the firſt of them may paſs over the center of the glaſs, and move exactly along one of the threads, whilſt the interval of time between its tranſit and that of the following ſtar, is accurately meaſured by a pendulum-clock, well adjuſted to the mean motion of the ſun or elſe to the revolution of the fixed ſtars, whereby the difference of right aſcenſion is given; as is the difference of declination, by the time the following ſtar takes to paſs from one diagonal thread to the other: By this manner of obſerving Dr. *Pound* and Mr. *Bradley* did, in an oppoſition of the *Sun* and *Mars*, demonſtrate the extreme minuteneſs of the *Sun*'s parallax; and that upon ſeveral repeated trials it was no more than 12″ nor leſs than 9″: But Dr. *Halley* conſidering, that in *October* 1721 *Mars* would be again in oppoſition to the *Sun*, about the 10th degree of *Taurus*; but not come very near any fixed ſtar, that has a place in Mr. *Flamſtead*'s *Catal.* he was ſolicitous to ſee, if there were any teleſcopic ſtars to which he would very nearly approach; and on the 28. of *February* 1720, the heavens being very ſerene and clear in the evening, and *Venus* having nearly the declination in which *Mars* moved *October* 1721; he fixed his teleſcope on her at 7^h 28′ equal time, and noted the moment ſhe had paſſed over the center of his glaſs, or rather the common interſection of the four croſs hairs; and in ½ an hour's time he obſerved eight very conſpicuous ſtars, four of which, being within the compaſs of one degree, fell very nearly in the ſaid way of *Mars*, and from the intervals of time he then obſerved, with their difference of declination from *Venus*, he determined their right aſcenſions and declinations, as well as her place from his tables (which by obſervation he found at this time needed no correction) would allow him; they all falling between the 9th and 10th degree of *Taurus* with very little Lat. But what confirmed him that all was right was, that on *March* 22. 1720, *Mercury* appearing very fair and juſt paſt his greateſt elongation, he found by *Senex*'s zodiac, that he was nearly in the ſame parallel,

Venus

Venus had before deſcribed; and tho' the brightneſs of the *crepuſculum* effaced the ſmaller ſtars; yet in a quarter of an hour he had one paſt 10′ and ¼ more ſoutherly than the planet, which in leſs than 3′ of time was ſucceeded by another, which was but 1′ more northerly than the former; when after an interval of about 14 minutes of time, in which he was ſurpriſed to find the ſky ſo void of ſtars, the four above-mentioned ſtars paſſed ſucceſſively over his glaſs, with the ſame interval of time in which he had ſeen them follow each other on the 28. of *Feb.* whereupon he was deſirous to try, whether if the place of *Mercury* in his tables were aſſumed, the ſame right aſcenſions and declinations of thoſe ſtars would be deduced from him, as from *Venus*; and the Dr. found on trial by an exact calculation, that he had the ſame right aſcenſions now as before, in none of the four differing quite ½ a minute; ſo that thoſe ſtars may be ſecurely added to the *Catal.* and the appulſe of *Mars* to them be obſerved in very long teleſcopes in *October* 1721. to the farther aſcertaining the immenſe diſtance between the ſun and earth.

Hence it will likewiſe appear that the Dr's. mercurial numbers are at leaſt at this time, and in this part of his orb, not leſs exact than thoſe of *Venus*; and whereas this planet ſcarce ever appears with us out of the ſun's beams, and always low, and therefore, under great refraction; this way of obſerving takes off all the uncertainty that accrues therefrom; and when once the zodiac ſhall be compleated with the ſtars that are wanting to fill up the vacant places, it will be eaſy at any time by this method, to obſerve *Mercury* or a comet within the ſun's beams, with the ſame certainty, as if it were remote, and out of the neighbourhood of the horizon, where the different vapours that lie near the earth, render the appearances of the ſtars ſomewhat dubious upon account of the irregular refractions.

A Propoſal for meaſuring the Height of Places by help of Mr. Patrick's Barometer, *in which the Scale is very much enlarged; by the Same.* Phil. Tranſ. N° 366. p. 116.

SINCE *Torricelli* firſt found the mercury in an inverted tube to be *in æquilibrio* with the whole column of air over it, and the weight of the incumbent column to be various according to the different diſpoſitions of the air, in reſpect of ſerene fair weather, and of rainy, windy or otherways tempeſtuous weather; there have been ſeveral attempts and contrivances to make the minute variations thereof more ſenſible: And firſt the wheel barometer was thought of, which certainly ſhews theſe variations with

with great exactneſs; but it is only proper for a fixed ſtation, and not eaſy to be removed; which circumſtance is requiſite for the principal uſe, to which this inſtrument is applicable, and for which Dr. *Halley* would recommend it.

The next thought for this purpoſe was that of Mr. *Hubin* (as deſcribed in *Phil. Tranſ.* N° 184.) who returning the tube of the barometer, as an inverted ſiphon, made a large dilatation in the aſcending leg thereof, wherein the mercury aſcended, as its altitude in the other part thereof abated, and *è contra:* Over this he drew out a narrow glaſs-tube, which he filled with tinged ſpirits, and which, being about 15 times lighter than mercury, would aſcend about 15 times as much as the mercury in the barometer fell. This, beſides that the ſpirits would dilate and contract with heat and cold, had the inconveniency of the former, of not being eaſily removed without great danger of diſordering and breaking it, by reaſon of the ſmallneſs of the tube in which the ſpirits were to riſe and fall.

This was ſucceeded by Dr. *Hook*'s marine barometer, made of two thermometers, the one, the common ſealed weather-glaſs, having no communication with the external air, wherein the temperature as to heat and cold was ſhewn by the ſwelling or ſhrinking of the included ſpirits; the other, the old thermometer, made with an inverted bolt-head, in whoſe globular part was included air ſomewhat rarer than the circumambient; ſo as to make the liquor, which was to riſe and fall in the ſhank of the bolt-head, always to ſtand above the ſurface of the ciſtern, into which its extremity was immerſed. This ſhewed the heat of the air by its dilatation, but at the ſame time the different preſſure of the atmoſphere mixed with it; ſo that the graduation of theſe two thermometers being adjuſted to any given height of the mercury, they would at all times, when the mercury was at that height, both ſhew the ſame degree of heat: But at other times, when the weight of the air was different, that difference would ſhew itſelf by the diſagreement of the degree of heat ſhewn by them. This will be better underſtood from *Phil. Tranſ.* N° 269, wherein this inſtrument is deſcribed at large. This tho' of admirable uſe at ſea, to give timely notice of approaching bad weather, labours under this objection, *viz.* that it ſuppoſes the concave of the tubes of the thermometers to be cylinders, or of equal diameters throughout; and alſo, that on account of heat and cold, the air and ſpirits have a proportional dilatation and contraction; the firſt of which the Dr. takes to be very hard to be found in ordinary

ordinary-glaſs tubes; and the other ſtill wants to be made out by authentic experiments.

The laſt contrivance for this purpoſe is that of Mr. *Patrick*, who calls himſelf the *Torricellian* operator, by filling a ſmall glaſs-tube about five foot long, and ſomewhat (but as little as poſſible) tapering upwards towards the cloſe end of the tube; then inverting it without a ſtagnant ciſtern of mercury, ſo much of the mercury as exceeds the length of the column the atmoſphere can at that time ſupport, will drop off, and leave its length equal to the then height of the common barometer: Now when the barometer riſes, this length in the tube becomes greater by the mercury's being preſſed up into the upper and narrower part of the tube; and when it falls on the contrary, it ſettles down into the wider part thereof and becomes ſhorter, being always the ſame in quantity. By this means, as the angle of the concave cone of glaſs, of which this tube conſiſts, is ſmaller, the different ſituation of the mercury will, upon the alteration of the preſſure of the air, be nicely ſhewn by very large and diſtinct diviſions.

Now the uſe to which the Dr. would apply this contrivance of the barometer is, to meaſure thereby the different levels of places too remote to be come at by the ordinary inſtruments for levelling with the certainty one would deſire. For this purpoſe let there be provided two ſmall glaſs-tubes nearly ſimilar, tapering but very little at the cloſe extremity; ſo that being inverted, the mercury may be ſuſpended in them at the height it ſhould have at the time of the experiment. Let that height be duly noted, and then going up the *monument*, or ſome ſuch edifice where the aſcent may be exactly meaſured, let the ſcales annexed be divided into parts by the deſcent of the mercury, at every 10 foot, in both the pendent barometers; which he conceives may be choſen in ſuch a manner as to make the diviſions very diſtinct and ſenſible. Theſe being thus prepared, when it is deſired to take the level of two diſtant places, let one of the glaſs tubes be placed in the lower place, at a time when the mercury has the ſame height, as when they were firſt inverted and graduated; and let the other be carried to the higher place, where it will be found to ſtand at that diviſion which anſwers to the height of that place above the other, which had been before found by meaſure in going up the *monument*: Thus may 90 foot aſcent, which makes but one tenth of an inch of mercury, be repreſented by two or three inches, or a ſpace capable of being divided into 90 parts; whereas,

whereas, if the distance of the two places be 20 miles, a minute of a degree is equal to above 30 foot; and by the usual sights, whether telescope or otherways, of water-levels, the Dr. is apprehensive it will be very difficult to convey a true level without a greater error than one minute in the whole.

The Variation of the Needle, *in the* Baltic; *by Mr.* William Sanderson. Phil. Trans. N° 366. p. 120.

Wednesday June 1. 1720, being at anchor near *Revel* in 58° 58′ N. Lat. the magnetical amplitude at sun-set was

west ——	64°	30′	N.
The true amplitude was W. ——	49	37	N.
Variation N——	14	53	W.

Saturday July 23. at the *Isle of Gottsand* in 58° 21′ N. Lat. at sun-set, the magnetical amplitude was W. 49° 50′ N. and the true amplitude W — — 35 00 N. which gives the variation N — — 14 50 W.

The difference of Long. of the two abovementioned places by *dead reck.* is 1° 50′.

October 24. At *Bornholme* in 56° Lat. at sun-rising, the magnetical amplitude was — E. 43° 15′ S. and the true Amplitude was — E. 28 31 which gives the Variation — — N. 14 44 W.

A large Quantity of Alcalious Salt *produced by burning rotten Wood*; *by Mr.* Robie. Phil. Trans. N° 366. p. 121.

A White oak-tree, about two foot diameter, in *Cambridge* in *New-England*, is of so surprising a nature, that tho' about ½ of it were decayed, and seemed to be really rotten wood; yet this decayed part would in burning turn almost entirely into a good white *alkali*, and run down into hard, white and clean lumps. Upon tasting of the lumps it was found to be salt and very strong; dissolved in clean water, upon decantation and evaporation without filtring, a very clean white salt was produced exceeding in strength and whiteness any had at the shops.

Now tho' *Alkali's* may be extracted from common aſhes; yet what was peculiar in this, is 1. That while it was burning, the wood itſelf would melt and run down into hard lumps of ſalt, not the ſound, but only the decay'd wood would do this; and what was moſt decay'd, would yield the greateſt quantity of ſalt. 2. Whereas all other *Alkali's* of wood, made thus by incineration, are blackiſh at firſt, and a *lixivium* made of them, tho' often filtred, will ſtill be tinged with a brown colour, occaſioned by a kind of coal or aſhes, ſo cloſely united to the *alkali* in burning, as not to be eaſily ſeparated by filtration, tho' often repeated; yet this *alkali* was very white; even before ſolution; and when diſſolv'd, the *lixivium* was not in the leaſt tinged, but clear like pure water, only a very ſmall quantity of aſhes ſubſided to the bottom of the veſſel, in which the ſolution was made. The *lixivium* thence decanted needed no filtration, but when boil'd up *ad ſiccitatem*, the ſalt remained fine and white. 3. That in the burning of this wood, as the heat of the fire grew more intenſe, the wood melted as it were and clodder'd together in large lumps; and viſibly bubbled and boil'd with an hiſſing noiſe like the frying of fat in a pan. 4. That whereas the weight of the alkalious ſalt produced from other wood, in the common way of incineration, is very inconſiderable, in proportion to the weight of the wood that produces it; yet this ſalt nearly equalled in weight the wood from which it was taken. 5. Whereas the aſhes of other wood are never ſo replete with ſalt, as that it can be obſerv'd, or in the leaſt cauſe the aſhes to lump or clod together; yet the whole of this would gather into hard and ſolid lumps of white ſalt, as eaſily to be diſtinguiſhed from aſhes, tho' white, as the pureſt ſalt of tartar, made with nitre, would be. 6. That tho' a much leſs quantity of an *alkali* can be produced from other rotten wood than from ſound wood; yet here it is quite the contrary, the decay'd part of this tree yielding a large quantity as aforeſaid; and the other or ſound part yielding no more than other wood.

Mr. *Robie* having examined the tree and conſider'd what (by the marks found thereon) probably happened to it, firſt premiſes, that in all likelihood it was ſtruck with lightning ſeveral years before; it being torn from the top of the trunk to the bottom on that ſide which was decay'd, and yielded the aforeſaid ſalt, there being a channel from top to bottom about five inches wide, as he ſuppoſes, at firſt, and which length

of

of time had clos'd up; and under this bark, the wood next to it was black, which he ſuppoſes, was caus'd by lightning.

Hence he conjectured, that the wood being thus expoſed to the air and water for ſo long a time, was the occaſion of its becoming defective in that part; and that the lightning having penetrated the wood, had ſo alter'd and diſpos'd its parts and pores (the figure and texture of the parts appearing very different from other rotten wood) to attract, imbibe and retain the nitrous ſalt of the air, which in ſo long a time, muſt have been very copious: Even as ſalt of tartar or other *alkali's*, expos'd to the air for ſome conſiderable time will be entirely reduced to a nitrous ſalt, as *Glauber* affirms, and its quantity likewiſe conſiderably increas'd, not that the lightning had calcin'd the wood in ſuch a manner, as to reduce it to a perfect ſalt; yet by penetrating into it, it had calcin'd it ſo, as to impart to it a like property or diſpoſition of attracting the nitrous ſalt of the air, as the aforeſaid *alkali's* of wood that have been perfectly calcined.

Now if it ſhould be objected, that the nitre in this wood being volatile would fly away in the burning the wood: To this Mr. *Robie* anſwers, that tho' nitre cannot be fix'd and reduced to an alkalious ſalt, by calcining it *per ſe*; yet it may, by the addition of the powder of charcoal, as chemiſts tell us; and here he ſuppoſes the wood ſo alter'd by lightning in which this nitrous ſalt was lodg'd, as to ſerve inſtead of coal in the burning of it.

An Account of a Foetus *that continued 46 Years in the Mother's Body; by Dr.* Steigerthal. Phil. Tranſ. N° 367. p. 126.

ANNA *Mullern* of the vilage of *Leinzelle* near *Gemund* in *Suabia*, of a dry and thin conſtitution, but otherways healthful and robuſt, died at the age of 94, after ſhe had liv'd a widow 40 years.

Six and forty years before her death, ſhe declar'd herſelf to be with child, and ſhe had all the uſual ſigns of pregnancy: At the end of her reckoning the waters came away, and ſhe was taken with the pains of labour, which continued upon her about ſeven weeks, and then went off upon uſing ſome medicines given her by a ſurgeon. Some time after, ſhe recover'd her perfect health, only that her belly continued ſwell'd, and that now and then upon any exerciſe, ſhe felt ſome pain in the lower part thereof: She was after this twice brought to bed;

the first time, of a son, and afterwards of a daughter: But notwithstanding this, she was firmly persuaded, that she was not yet deliver'd of what she first had gone with; and she desir'd Dr. *Wohnlixe* the Physician of *Gemund*, and one *Knaussen*, a surgeon at *Heubach*, to open her body after her death: Accordingly dying on the 11th of *March* 1720 after four days illness, her body was opened by the surgeon, who found within her a hard mass of the form and size of a large ninepin bowl, without observing whether it lay in the *uterus* or out of it.

A Fig. 1. Plate IV. represents a part of the integument, which adher'd to a spongy fleshy substance; this at first seem'd to be a mass of cartilage, but was afterwards found to be entirely bony; B B B C C C shews the membranous part, which was bloody; D D D D the opening made by the hatchet, which was the instrument made use of; E E E E E another part of the integument appearing entirely bony, with several prominencies; F a contusion occasioned by the manner of opening it, where there appeared some putrified membranes.

A A A Fig. 2. represents the integument or substance inclosing the *fœtus*; B the *fœtus*; C a depression or hollowness on the right cheek; α the nose turning up; β the mouth flatted; but not so wide, as here represented; γ the eye clos'd up; δ the ear; D D the arms, of which the right was the larger, and the two joints of the right thumb were plainly to be seen; E the protuberance of the knee; F part of the *funiculus umbilicalis* torn, but still adhering to the navel; G G part of the same *funiculus* adhering to the bony part of the integument; H the breast; I the mark of an incision into the left side, where the flesh appeared red, but was dry and look'd like smoak'd beef.

Fig. 3. represents two ribs from the left side, of their natural substance, colour and bigness; A A the part joining to the *vertebræ*; B B the part joining to the *sternum*.

This is preserv'd in the cabinet of rarities of the Duke of *Wirtemberg*.

Dr. *Camerarius*, Professor at *Tubingen*, in a letter on this subject, takes notice, that the surgeon found this mass in a cavity on the woman's left side, and that it adher'd to the membranes of that cavity by the intervention of a spongy fleshy substance: From which particular, and the woman's having had two children, during the time that this large mass lay in her, that learned Professor conjectures, that it was not lodged in the womb, but in the left *tuba Falloppiana*, which by

by this means, had been very much dilated and thicken'd in its ſubſtance.

Obſervations on the Membranes, encloſing the Faſciculi *of Fibres, into which a Muſcle is divided; by M.* Leewenhoeck. Phil. Tranſ. N° 367. p. 129.

M. *Leewenhoeck*, in cutting off ſeveral thin ſlices from a piece of beef, whenever he cut the fleſhy fibres thro' tranſverſely, could plainly diſcover the membrane (as it is commonly call'd) which runs between and envelopes the fleſhy fibres, and eſpecially the larger *faſciculi* of them, as they run lengthways along the muſcle: Between theſe *faſciculi* the membrane is of a conſiderable thickneſs, but ſpreads out every way into exceeding ſmall ramifications. Some weeks before, he obſerv'd, that this membrane was compos'd of an inconceivable number of very minute veſſels, which were plainly to be diſcern'd, not only where the membrane appear'd of ſome conſiderable breadth, but even where it was not ſo broad as a ſingle muſcular fibre; but how far this held, he could not determine, foraſmuch as theſe ſmall ramifications of the membrane did again ſpread themſelves into other ramifications ſo exceeding fine, eſpecially where they inclos'd the ſingle muſcular fibres, that they were in a manner inviſible, even thro' his beſt microſcopes.

The very ſmall veſſels compoſing this membrane (as it is call'd) are doubtleſs deſign'd to convey ſome nutritious juices: Yet they are ſo ſmall, that the globules of blood cannot paſs thro' them.

A B C D Fig. 4. Plate IV. repreſents a ſmall piece of the membrane, which, with the adjoining fleſhy parts, is cut thro' tranſverſely; and it being impoſſible to draw the vaſt number of veſſels, which compoſe it, on account of their exceeding minuteneſs, they are repreſented only by points; E F G and H I repreſent the carnous fibres cut thro' tranſverſely along with the membranes.

Theſe carnous fibres, when moiſt, lay ſo cloſe to each other, that the ſpace between E F G and H I was quite fill'd up; but when dry, they were ſo ſhrunk up, that one might obſerve ſuch intermediate ſpaces, as in the figure.

Now as we ſee, upon the drying of the membranes A F G and D E G, with the muſcular fibres between them, what number of ſmall ramifications proceed from the membranes, as is repreſented between the muſcular fibres; we muſt not imagine

gine, that theſe ramifications proceed only from the points here repreſented; but that they are continued the whole length of the fibres; and ſubdividing themſelves into ſtill finer ramifications, they incloſe every ſingle fibre in the whole muſcle.

Among ſeveral pieces of fleſh, where the carnous fibres were cut tranſverſely, M. *Leewenhoeck* happened on one piece, with its branches ſo plain, that the membranes and fibres reſembled ſo many boughs of trees with the leaves on, as repreſented by K L M N Fig. 5. where M ſhews the membrane torn off from another, as alſo the number of branches it runs into, and the ſeveral fibres it covers.

All theſe carnous fibres, with their membranes, lay very compact together, when he cut them off from the pieces of fleſh; as alſo when he laid them upon the glaſs and moiſtened them; but as the moiſture evaporated, they ſhrunk again, in the manner here repreſented; and tho' the deſigner could plainly diſtinguiſh the ſmall veſſels cut thro', the largeſt of which appeared at M, yet he was obliged to mark them with points only. Here you may obſerve, that all the carnous fibres, being cloſely tied together by the ſaid membranes, by which they were enveloped, and which are nothing but a congeries of veſſels, could not be ſeparated from each other upon drying, but by tearing thoſe membranes aſunder.

The carnous fibres together with the membrane at K L M N Fig. 5. do not take up ſo much room, but that a grain of ſand may cover it; and yet in ſome of thoſe carnous fibres, one might very diſtinctly obſerve, the parts of which they were compos'd.

He purſu'd this obſervation in the fleſh of a whale, of which he had two pieces by him, for about ſeven or eight years, of a ſpan long and two inches thick; from theſe he cut ſeveral ſlices tranſverſely, but found the carnous fibres, ſo cut thro', eaſily ſeparate from each other; ſo that he could not find his account in this, but thought the membrane was rotten; he therefore cut off the outſide with a table-knife, and then with a very ſharp knife he cut the inner part into very thin ſlices; and there he found the excrements of mites, very ſmall, but globular; and ſome of them as ſmall as ever he had obſerv'd; and theſe excrements he found every where, eſpecially where the membranes were thickeſt; then viewing the parts where the membranes were thinneſt, he there diſcovered the aforeſaid veſſels, in as great number as he had obſerv'd them in an ox's fleſh, and as diſtinctly.

After

After the former diſcoveries as to the circulation of the blood; particularly, that the blood-veſſels have no terminations, he began to conſider how the fat particles could be form'd; ſince he did not think, they were ſeparated from the blood and emitted from the blood-veſſels: But having now plainly diſcovered, that theſe membranes were nothing but very ſmall veſſels, and ſuppoſing that they were form'd for no other end, but to convey nutriment; as alſo that there was no circulation in theſe veſſels, he imagined, that the matter call'd fat, was brought into them; which, when there was too great a ſupply of nutriment, ſo as that it could not be farther propelled, muſt be driven out of theſe veſſels; for, all the particles of fat he hitherto obſerv'd, were inclos'd in ſmall films.

This original of the fat ſeems more probable, than that it ſhould be forced out of the blood-veſſels; and yet how theſe fatty particles are form'd, which conſiſt of ſmall globules, and theſe of ſtill ſmaller globules, he could not hitherto determine; as alſo where theſe veſſels, which conſtitute theſe membranes, have their origin and how this fat is convey'd into them.

M. *Leewenhoeck* had in his drawer a piece of ox fleſh, that had lain there about four years, wrapp'd up in a paper; and he found this piece of fleſh in ſome places covered with a membrane; from this he cut off ſeveral ſmall ſlices along with the membrane; and he found, that there lay near the membrane about 16 or 18 nervous fibrils, which, in the drying of the fleſh were ſo ſqueez'd together, that they were almoſt twice as long as broad; in ſome of which he obſerv'd very diſtinctly thoſe veſſels, which are in the nerves.

Theſe nervous fibrils were inclos'd in a ſort of half-round, which ſeparated them from the muſcular fibres, and which conſiſted of a row of ſmall tendinous fibrils, each of which was about twice as thick as the hair of a man's beard; without theſe tendinous fibrils by the muſcular fibres, which had been cut thro' tranſverſely; and in this part of the half-round there were ſeveral apertures, which ſeem'd in the microſcope to be big enough for a grain of hemp-ſeed to paſs thro', and might well be taken for veſſels, but that there lay ſo many of them together: But conſidering, that the nerves are commonly cover'd with fatty particles, he concluded, that theſe apertures were no veſſels, but meer fatty particles, which he found to be ſoupon cutting thro' them; and diſcovered that the inner fat was eaten out by the mites, which had left only the huſks

or

or *cortices* of the fat globules behind; which *cortices* he could never hitherto discover; because they would, upon any heat, melt away as fast as the inner fat itself.

Observations on the Vessels in several Sorts of Wood *and on the muscular* Fibres *of different* Animals; *by the Same.* Phil. Trans. N° 367. p. 134.

M. *Leewenhoeck*, having procured a piece of wood (brought from the island of *Amboyna* in the *East-Indies*, and of which cabinets are made) sawed off at the end of a board, as also some of the chips in order to observe the vessels therein; and cutting the wood all manner of ways, he found, that in one place it appeared whitish, at a small distance red, and in another place blackish. Upon cutting it transversely, he observed the orifices of the ascending vessels, which ran along the length of the wood, and which appeared of such a size in the microscope, that one would have judged a pea might pass thro': Where the wood looked reddish, he found these large vessels filled with a substance of a fine red colour; so that he imagined, these large vessels conveyed a red sap into the horizontal vessels, which appeared so very numerous and so thick together, that they caused the wood to seem of the same colour with the red substance, contained in them.

He afterwards cut off some very thin slices transversely from this wood, and putting them into a china-cup, he pour'd some hot water upon them and suffered them to lie therein for some time; then viewing them with a microscope he observed, that the red substance was extracted by the water, and no red colour was now to be found in any of the vessels.

What seemed most surprising to him in this wood was, that upon cutting thro' it lengthways, as he frequently did, he observed it to be of a fine red colour for a hair's breadth; and a hair's breadth farther it appeared white; and the ascending vessels seemed to be smaller, where the wood was red than where it was white; which narrowness of the red vessels he judged to be owing to the sap contained in them.

In viewing the ascending vessels in oak, he found some other vessels, which entered into their sides and appeared to him like so many small round holes; especially, where the horizontal vessels lay, which he judged to be united to the ascending vessels, by means of those small orifices, and thereby to discharge part of their sap into them.

M. *Lee-*

M. *Leewenhoeck* taking a ſmall twig of an oak, which in ſeven years growth was about the thickneſs of one's finger, cut it thro' lengthways both in the aſcending and horizontal veſſels, which laſt he obſerved lying in great numbers very cloſe together, and iſſuing directly from the pith of the twig.

He likewiſe made ſome obſervations on fir-wood, in which the aſcending veſſels conſiſt of ſo very fine and thin a ſubſtance, as to exhibit a very agreeable ſight in the microſcope: In theſe aſcending veſſels he imagined, he ſaw ſome globules, with a ſmall opening in their middle, which ſeemed to be of a cloſer and denſer ſubſtance than the reſt of the wood; but afterwards he found himſelf miſtaken, and that theſe ſuppoſed globules were nothing other than the orifices, by which the aſcending and horizontal veſſels were united together, and thro' which the ſap was conveyed from the one to the other.

From theſe obſervations he turned his thoughts to the fleſhy fibres of animals, and began to conſider with himſelf, that ſince the author of nature uſually obſerves the ſame frame and ſtructure in a great variety of his creatures; perhaps, the fine membranes, with which every muſcular fibre is inveſted, and which are provided with a vaſt number of ſmall veſſels, might convey nouriſhment in the ſame manner, thro' every carnous fibre in a healthful body.

In this view he cut off ſome very ſmall thin ſlices from the fleſh of an ox, directly acroſs the length of the fibres, and placing them upon glaſſes, and moiſtening them with clean rain-water, he obſerved them with a very good microſcope, and continued viewing them ſo long, till the fleſhy fibres began to grow dry: He then obſerved, that in ſome places the exceeding ſmall and fine veſſels, which compoſed the membranes that incloſe the fleſhy fibres, were broken off from the ſaid fibres, by the unequal ſhrinking of the thin ſlice of fleſh upon the plate of the microſcope. At the ſame time he obſerved ſome other of theſe ſmall veſſels, which were ſomewhat ſtronger than the former, and were not broken off from the fleſhy fibres; but yet were ſtretched and drawn from them to the diſtance of the diameter of a blood globule. He likewiſe obſerved ſome fleſhy fibres, which adhered ſo cloſe to other fibres, that the ſmall veſſels of communication were not broken off or ſtretched; ſo that nothing was to be ſeen there, but only the membrane encompaſſing the fibres.

He likewiſe placed before the ſame microſcope ſeveral other fleſhy fibres, which he had ſeparated lengthways from the fleſh

of an ox: In each of these he observed a great many small apertures, by which he judged, that the small vessels of the membranes had entered the fibres; and moistening them with water, no sooner was the little moisture, left in those apertures, evaporated then he could see them very plain and distinct.

In order to examine into the truth of this hypothesis, M. *Leewenhoeck* pricked his thumb with a fine needle, and placed a little blood upon the glass, where the fleshy fibres lay, in order to observe with his microscope the proportion between the diameters of a globule of blood and of the aforesaid apertures, which he had observed in a fibre. And it was found that the diameter of a blood-globule was four times as large as that of one of these apertures; and if so, then according to the known rule, a globule of blood must be divided into 64 parts, before it can enter one of these apertures in a fleshy fibre.

This discovery seemed very surprising to M. *Leewenhoeck*; and he is apt to think, that it will be very difficult to penetrate any farther into the hidden structure of the muscular fibres, and the manner in which they receive their nourishment.

M. *Leewenhoeck* having in a former *Transaction* asserted, that the muscular fibres are composed of long small filaments, he was sollicitous to discover whether these small filaments, which compose a carnous fibre, might not really be so many small vessels: With this design he took part of the flesh of a whale, he had kept some years by him; and cutting it into very thin slices directly across the fibres, and having moistened these thin slices with fair water, he placed them upon several glasses, and before several microscopes; when he observed, that what he had formerly taken for small threads or filaments, were in reality exceeding small vessels. He then cut part of the whales's flesh lengthways, in order to discover the vessels, which convey the nutritious juice out of the membranous into the muscular fibres; which vessels then appeared to him very numerous and very distinct.

He afterwards took another piece of the flesh of a very fat ox, which he cut thro' transversely, and looking upon it with some of his best microscopes, he could plainly see, that how small soever these fibres were, they were still vascular; for, he could see the light thro' the apertures of these vessels, as he had done before in those of a whale; but if he happened to cut the fibres never so little obliquely, instead of cutting directly

directly acroſs their length, the light was not to be ſeen thro' them.

He had in a drawer the hinder quarter of a mouſe, that had lain there ſome years; from the largeſt muſcle of which he cut off tranſverſely ſome ſmall ſlices, as thin as poſſible: Then placing theſe before his microſcope, he not only obſerved, that the carnous fibres were of the ſame thickneſs with thoſe of an ox; but beſides, he could obſerve the apertures of the veſſels, compoſing the carnous fibres, as plainly as in the fleſh of a whale: The veſſels in the muſcular fibres of a whale are, it is true, ſix times more in number than in thoſe of an ox or a mouſe, but then the fibre of a whale is alſo ſix times as thick as the other.

Hereupon he conſidered after what manner the veſſels, of which the muſcular fibres moſtly conſiſt, received their nouriſhment from the veſſels of the membranes; ſince the muſcular fibres when at reſt have ſeveral alternate corrugations, by which he judged, that the veſſels in the fibres muſt have their ſides preſſed together and their cavities cloſed up: But if we recollect, that in walking a man may move both his feet upwards of 3600 times in an hour (for, he may make two ſteps in the time of one pulſation of the artery) and that in that ſpace of an hour the muſcular fibres muſt be ſo many times dilated and contracted, and will therefore, require conſiderable ſupplies; we ſhall likewiſe find, that this is ſufficiently provided for, ſince upon every extenſion of the muſcle, the apertures of thoſe ſmall veſſels are free and open for the conveyance of nouriſhment into the fibres.

Experiments relating to the reſiſtance of Fluids; *by Dr.* Deſaguliers. Phil. Tranſ. N° 367. p. 142.

ON the 30. of *March*, 1721. Dr. *Deſaguliers* took a ball of gold an inch in diameter, that had a ſmall ſtem of the ſame metal, with a place thereon to faſten a ſtring to; and having ſuſpended it by a ſilken thread, too ſtrong to lengthen by ſtretching; he made the diſtance between the center of the ball and the point of ſuſpenſion, equal to 12,5 inches; then cauſing the ball to vibrate in a trough full of water (which had an upright piece of wood in the middle of one ſide, with pins or keys from which the ball hung, that the center of ſuſpenſion might always be in the ſame place) he obſerved by looking from a pin on one ſide of the trough to a mark made oppoſite to it on the other ſide, whereabouts the ſtring of the

pendulum (juſt above the ſurface of the water, in which the ball was quite immerſed) went after 14 vibrations; and by another pin and oppoſite mark, he alſo obſerved where it went to, after 28 vibrations. Taking out the water, he filled the trough with mercury; the length of the pendulum, the point of ſuſpenſion and all other things remaining as before; then letting go the ball in the mercury from the ſame place whence it was let down, when the trough was full of water (which was marked by a ſtring ſtretched acroſs to prevent miſtakes) after one entire vibration, it came very little ſhort of the ſame mark, as it came to in water, after 14 vibrations; and when it vibrated twice in mercury, it came to the ſame place it had done after between 26 and 28 vibrations in water; and thus it did exactly ſeveral times.

Afterwards filling with mercury an upright copper pipe of four inches diameter to the height of three foot 10 inches, and ſuſpending the golden ball therein by a ſhort ſtring about an inch long; ſo as to have the ball juſt immerſed under the middle of the ſurface of the mercury; he cauſed it to be let down ſuddenly, and obſerving how long it was falling down to the bottom of the tube, he found, that the experiment was diſturbed by the ball's ſtriking againſt the ſides of the tube, which retarded the fall of the ball, and the more ſo, the oftener the ball ſtruck. When the ball was leaſt retarded, it was only two ſeconds and $\frac{1}{2}$ in falling, which muſt be taken as the true time of the fall of the ball in a height of quickſilver, equal to 3 foot 10 inches; becauſe when he tried the experiment again at home the 1. of *April* following, the ball fell in the mercury once or twice without ſtriking the ſides of the tube at all, but not in leſs time than two ſeconds and $\frac{1}{2}$.

He alſo repeated the other experiments at home, making the golden pendulum 39,2 inches long, ſo as to make it vibrate but once in a ſecond; and then he found, that it would vibrate five or ſix times in the mercury, before the vibrations became ſo ſmall as not to be obſerved; and then the firſt vibration in the mercury ended very near where the 14th in water had done; the ſecond in mercury ended where the 27th in water had done; and obſerving the third vibration in mercury, it ended exactly at the mark, where the 40th in water ended; and this was obſerved by ſeveral perſons beſides.

Then the Dr. weighed 14 penny-weight of the mercury (in which he made the experiments) firſt in the air, then in water, where it loſt only one penny-weight and one grain of its weight;

weight; that is, it weighed in air 336 grains and in water 311; ſo that its ſpecific gravity was to that of water as 13,44 to 1.

As to the golden ball that had varniſh and cement upon it to keep the mercury from ſinking into it, he found it weigh as follows;

	oz.	*dwt.*	*gr.*	
In mercury	1	00	18 or	498 grains
In water	5	01	00 or	2424 gr.
In air	5	07	09 or	2577 gr.

Dr. *Deſaguliers* took the wire and pendulum of a long pendulum-clock; and having faſten'd the golden ball at the end of the wire under the pendulous weight that ſerved for the clock, in order to make the vibrations of the golden ball in the mercury continue longer; he did not find it keep on its motion above a ſwing or two the longer for it; nor did a round ball of lead, placed upon the ſaid wire, juſt above the ſurface of the mercury avail any more; and as he found ſome inconveniencies in theſe two laſt ways of making the experiment he rather choſe to rely upon thoſe made with the golden ball ſuſpended by a ſilken thread of 39,2 inches long, meaſuring from the point of ſuſpenſion to the center of the ball.

An Account of the Poiſon-wood-tree *in* New-England; *by Mr.* Dudley. Phil. Tranſ. N° 367. p. 145.

THE poiſon-wood-tree grows only in ſwamps or low wet grounds, and is ſomewhat like a ſmall aſh, but much more like a *ſumach*; and, therefore, is by ſome called the *ſwamp-ſumach*; for, its twigs, leaves and ſhape are exactly like the *ſumach*; and it likewiſe bears a dry berry.

It never grows bigger than a man's leg, nor taller than alder; but ſpreads much, and there are ſeveral together, eſpecially about the ſtump or roots of one that is cut down; as it is of a quick growth, ſo it does not laſt long; the inſide of the wood is yellow and very full of juice, as glutinous as honey or turpentine; the wood itſelf has a very ſtrong unſavory ſmell, but the juice ſtinks as bad as carrion.

As to its poiſonous quality; it muſt be obſerved 1. That it poiſons two ways, either by the touch or by the ſmell; the ſcent of it, when cut down in the woods or on the fire, being poiſonous to a very great degree: One of Mr. *Dudley*'s neighbours was blind for above a week together, with only handling it; and a Gentle-

Gentleman in the country, ſitting by his fire ſide in winter, was ſwelled for ſeveral days with the ſmoke or flame of ſome poiſon-wood, that was in the fire. 2. It has this effect only on ſome particular perſons and conſtitutions; for, Mr. *Dudley* has ſeen his own brother not only handle but chew it without any harm at all; and ſo by the ſame fire one ſhall be poiſoned and another not at all affected. 3. This ſort of poiſon is never mortal, and will go off in a few days of itſelf, like the ſting of a bee; but generally the patient applies plaintain-water, or ſallet-oil and cream. 4. As to its operation; within a few hours after the perſon is poiſoned, he feels an itching pain that provokes a ſcratching, which is followed by an inflammation and ſwelling; ſometimes a man's legs only have been poiſoned and run with water.

Mr. *Dudley*'s neighbour that was ſo badly poiſoned with handling it, told him one thing very remarkable of the wood, and that is, that when he touched it, he plainly perceived it to differ from the other wood he was throwing up into his cart; for, it was as cold as a piece of ice; and he withal aſſured him, he could diſtinguiſh it blindfold, or in the dark from any other wood in the world, by its coldneſs: He farther told Mr. *Dudley*, that he felt an itching in a few hours after he had handled the wood, but that the ſwelling did not come on till about three days after.

A farther Account of the ſame Tree; *by Dr.* William Sherard. Phil. Tranſ. N° 367. p. 147.

THE account Dr. *Sherard* had of the poiſon-tree from Mr. *More* (which probably he had from Mr. *Dudley*) is as follows.

The poiſon-tree grows to the bigneſs of an alder, the wood is as cold as ice; when laid on the fire, at which five or ſix perſons ſit, ſome will fall a ſwooning, fainting or yawning for ſome days; others only for a few hours; and others of the company are not at all affected: Mr. *More* could handle, cut and burn it without any hurt; and ſo it is, with ſeveral others, he ſuppoſes, according to their ſeveral conſtitutions: It was never known to kill any body, only to do hurt to ſome perſons.

Mr. *More* calls it a water-ſhrub: It is a ſpecies of *toxicodendron*, tho' not mentioned by Dr. *Tournefort* in his *Inſtitutions p.* 610; but Dr. *Sherard* takes it to be *arbor Americana alatis foliis, ſucco lacteo venenata. Pluknet. Almag.* 45. *Tab.* 145. Fig. 1. which is a ſpecies of *toxicodendron*, that grew formerly at *Chelſea* garden. What makes Dr. *Sherard* take it to be this

is,

Fig. I.

Fig. II.

Fig. III.

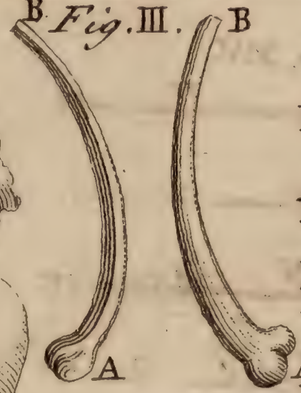

Fig. XIV.

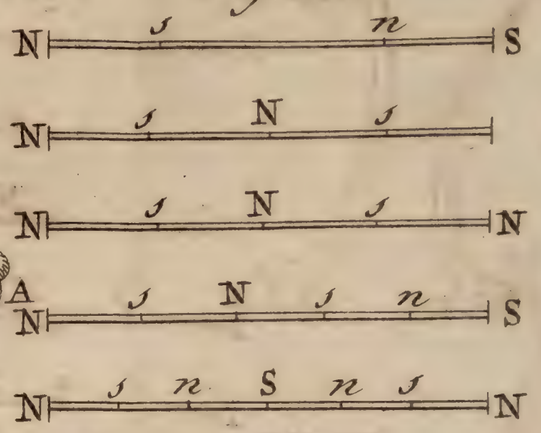

Fig. XV.

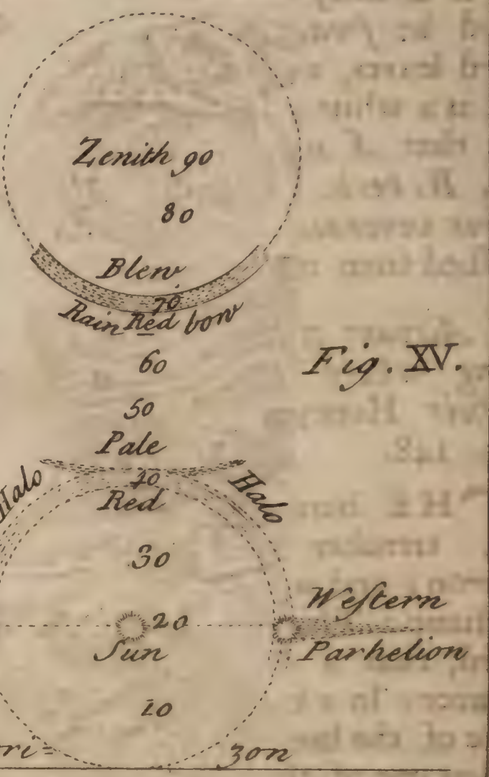

Fig. IV.

Fig. V.

Fig. VI.

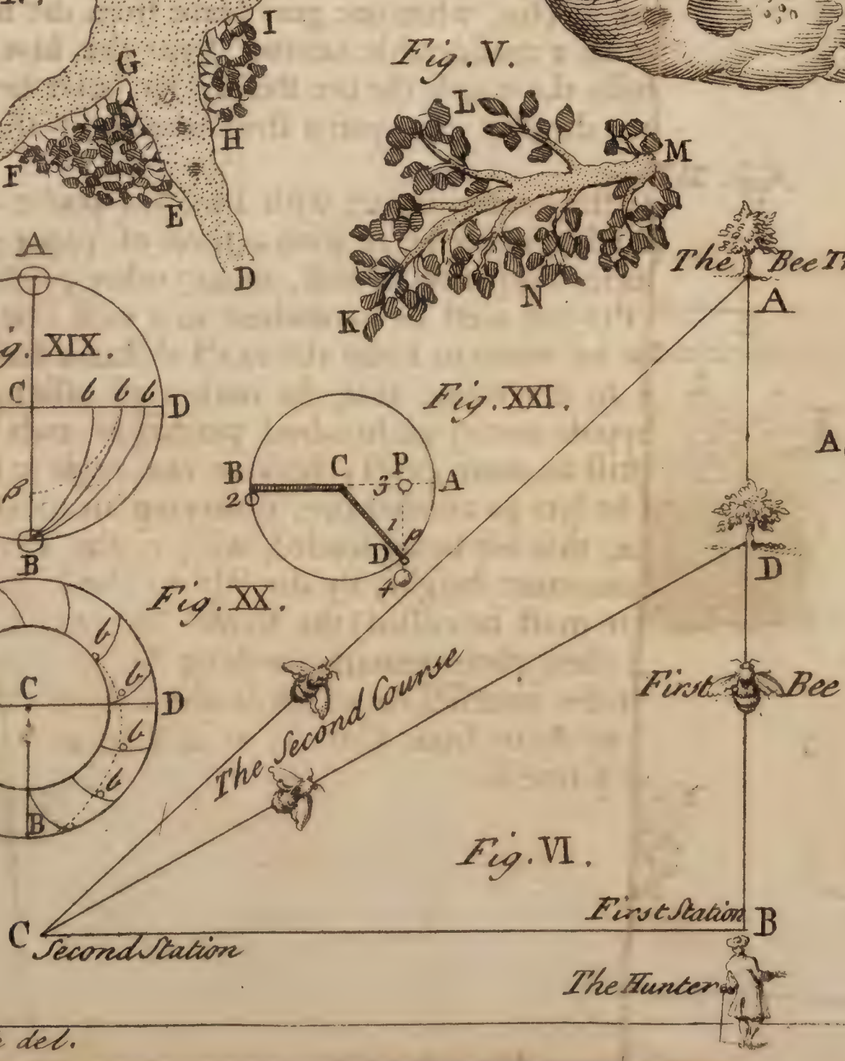

Fig. VII.

Fig. VIII.

Fig. IX.

Fig. X

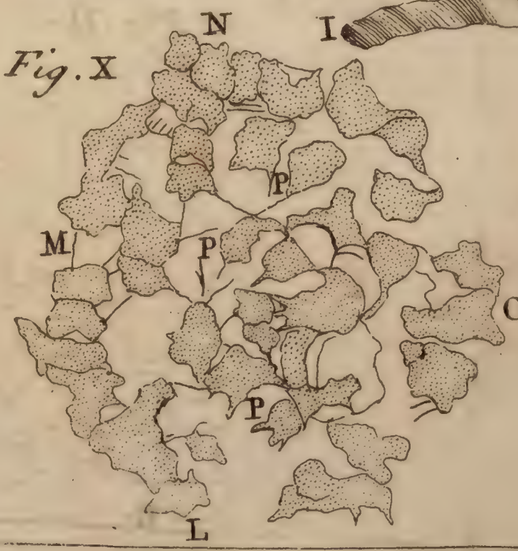

Fig. XI.

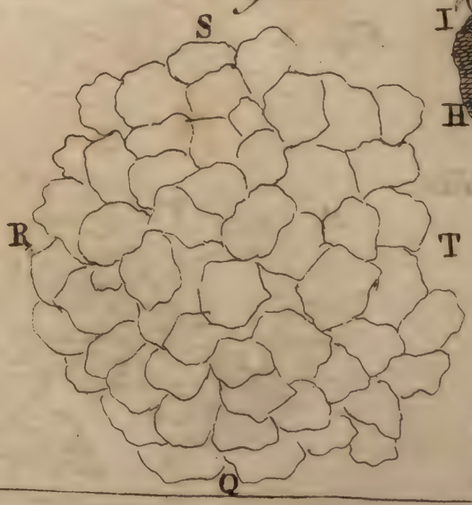

Fig. XII.

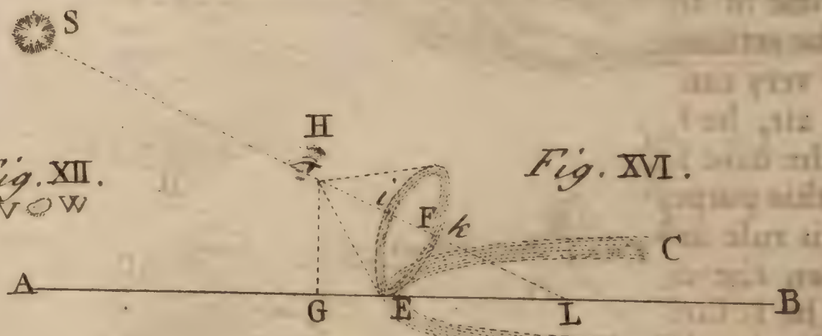

Fig. XIII.

Fig. XIV.

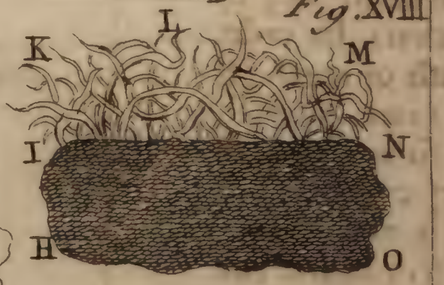

Fig. XVI.

Fig. XVII.

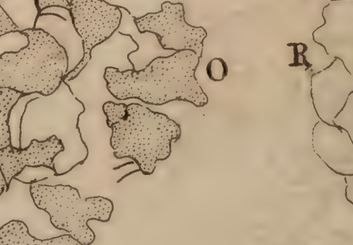

Fig. XVIII.

Fig. XIX.

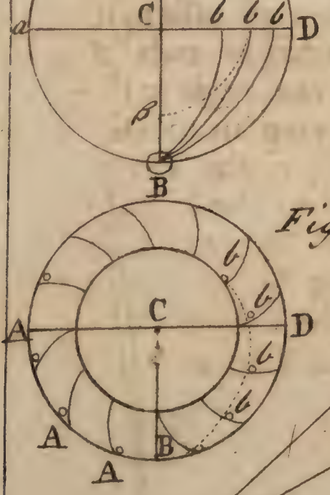

Fig. XX.

Fig. XXI.

is, Mr. *Dudley*'s saying it is like a *sumach*, and that it is by some called the *swamp-sumach*; this, in its manner of growing and alated leaves, very much resembles the *sumach* or *rhus*; the fruit is a white roundish dry berry, growing in clusters; and so, like that of *toxicodendron triphyllon folio sinuato, pubescente, Inst. R. herb.* 611. *Hederæ trifoliæ Canadensi affinis planta: Arbor venenata quorundam H. R.* Paris; as scarce to be distinguished from it.

An Account of a new Method in New-England *for discovering where the* Bees Hive *in the Woods, in order to get their* Honey; *by Mr.* Dudley. Phil. Trans. N° 367. p. 148.

THE hunter in a clear sun-shiny day takes a plate or trencher with a little sugar, honey or molosses, spread thereon; and when got into the woods, he sets it down on a rock or stump: This the bees soon find out; for, it is generally supposed, that a bee will smell honey or wax at above a mile's distance: In a box or other conveniency the hunter secures one or more of the bees, as they fill themselves; and after a little time, he lets one of them go (for, when one goes home from the sugar-plate, he returns with a considerable number from the hive) observing very carefully the course the bee steers; for, after he rises in the air, he flies directly on upon a strait course to the tree where the hive is.

For this purpose the hunter carries with him his pocket-compass, his rule and other implements with a sheet of paper; and sets down the course, supposing it west, or any other point, and by this he is sure the tree must be somewhere in a west line from where he is; but he wants to know the exact distance from his station: In order to determine that, he makes an offset either south or north (suppose north) an hundred perches or rods (if it be more, it will still be more exact; because the angle will not be so acute) then he lets go another bee, observing his course also very carefully; for, this bee being loaded, will, as the first (after he is mounted a convenient height) fly directly to the hive; this second course (as it must be called) the hunter finds to be south, 54 degrees west; then there remains nothing but to find out, where the two courses intersect or which is the same thing, the distance from B to A or from C to A, as in Fig. 6. Plate IV. for, there the honey-tree is.

For

For which reaſon, if the courſe of the ſecond bee from C had been ſouth weſt and by ſouth, *viz.* to D; then the hive-tree muſt have been there; for, there the lines are found to interſect.

All this is founded on the ſtrait or direct motion of bees, when bound home with their honey; and this is found to be certain by the obſervation and experience of the hunters every year; eſpecially, ſince this mathematical way of finding honey in the woods has been uſed with ſuch ſucceſs.

All the bees they have in their gardens or woods, and which now are in great numbers, are the produce of ſuch as were brought in hives from *England*, near a hundred years ago, and not the natural produce of this part of *America*; for, the firſt planters in *New-England* never obſerved a bee in the woods, till many years after the country was ſettled: But what proves it beyond diſpute is, that the *Aborigines* (the *Indians*) have no word in their language for a bee, as they have for all animals whatſoever proper to, or aboriginally of the country; and, therefore, for many years they called a bee by the name of *Engliſhman*'s fly.

They formerly uſed to find out honey in the woods, by ſurpriſing and following one bee after another by the eye, till at length they found out where the bees hived.

It is obſervable, that when bees ſwarm, they never go to the northward, but to the ſouthward, or to a point inclining that way.

An Account of the Mooſe-deer *in* America; *by the Same.* Phil. Tranſ. N° 368. p. 165.

THE *mooſe* is an animal, not only proper, but thought peculiar to *North America* and one of the nobleſt creatures of the foreſt: The *Aborigines* have given him the name of *mooſe*; *Mooſuk* in the plural.

There are two ſorts of *mooſe*; the common light *grey mooſe* and the large or black *mooſe*: The former the *Indians* call *Wampooſe*; they are more like the ordinary deer, ſpring like them and herd ſometimes to the number of 30 together.

The black *mooſe* is chief of the deer kind; has ſeveral things in common with other deer; differs in ſeveral things, but in all very ſuperior to them: The *mooſe* is ſhaped much like a deer, parts the hoof, chews the cudd, has no gall, his ears large and erect: The hair of the black *mooſe* is a dark grey; as alſo the ridge of his back to 10 and 12 inches long, of which the *Indians* make

make good belts; he has a very ſhort bob-tail: Mr. *Neal* in his hiſtory of this country, ſpeaking of the *mooſe*, ſays they have a long tail; but he is miſtaken as to his account of other things beſides the *mooſe*.

The hunters have found a buck or *ſtagg-mooſe*, 14 ſpans high from the withers, reckoning nine inches to a ſpan; a quarter of his veniſon weighed upwards of 200 pounds: A doe or hind of the black *mooſe* of the fourth year was killed within two miles of *Boſton*, which from the noſe to the tail meaſured between 10 and 11 feet; and wanted an inch of ſeven foot in height.

The horns of the *mooſe*, when full grown, are between four and five foot from the head to the tip, with ſeven ſhoots or branches to each horn, and they generally ſpread about ſix foot: When the horns come out of the head, they are round like an ox's horns; about a foot from the head, they begin to grow a palm broad and further up ſtill broader, of which the *Indians* make good ladles that will hold a pint: When a *mooſe* goes thro' a thicket or under the boughs of trees, he lays his horns back on his neck, not only that he may make his way the eaſier; but to cover his body from the bruiſe or ſcratch of the wood: Theſe huge horns are ſhed every year: The *doe-mooſe* has none of theſe horns.

A *mooſe* does not ſpring, or riſe in going, as an ordinary deer, but ſhoves along ſideways, throwing out his feet, much like a horſe in a racing pace: One of theſe large black *mooſe* has, in his common walk, been ſeen to ſtep over a gate or fence five foot high. After you unharbour a *mooſe*, he will run a courſe of 20 or 30 miles, before he turns about or comes to a bay; when they are chaſed, they generally take to the water; the common deer, for a ſhort ſpace, are ſwifter than a *mooſe*; but then a *mooſe* ſoon outwinds a deer.

The fleſh of a *mooſe* is excellent food; and tho' it be not ſo delicate as common veniſon, yet it is more ſubſtantial and will bear ſalting: The noſe is reckoned a great dainty; Mr. *Dudley* eat ſeveral of them and found them to be perfect marrow: The *Indians* told him, they could travel three times as far after a meal of *mooſe*, as after any other fleſh of the foreſt.

The *black mooſe* are not very gregarious, not above four or five being found together; the young ones keep with the dam a full year.

A *mooſe* calves every year and generally brings two: The *mooſe* bring forth their young ones ſtanding, and the young fall

from the dam upon their feet: The time of their bringing forth is generally in *April*. *Job.* cap. 39. v. 1, 2, 3. *Canſt thou mark when the hinds calve, or knoweſt thou the time when they bring forth? They bow themſelves, they bring forth their young ones, they caſt forth their ſorrows.*

The *mooſe* being very tall and having ſhort necks, do not graze on the ground, as the common deer, neat cattle, &c. do; and if at any time they eat graſs, it is the top of that which grows very high or on ſteep riſing ground. In the ſummer they feed on plants, herbs and young ſhrubs, that grow on the land; but moſtly and with greateſt delight on water-plants; eſpecially a ſort of wild colts-foot and lilly, that abound in the ponds and by the ſides of the rivers; and for which the *mooſe* will wade far and deep; and by the noiſe they make in the water, the hunters often diſcover them: In the winter they live on browſe, or the tops of buſhes and young trees; and being very tall and ſtrong, they will bend down a tree as big as a man's leg; and where the browſe fails them, they will eat off the bark of ſome ſorts of trees, as high as they can reach: They generally feed in the night, and lye ſtill in the day.

The ſkin of the *mooſe*, when well dreſs'd, makes excellent buff; the *Indians* make their ſnow-ſhoes of them: Their way of dreſſing it, which is reckoned very good, is this; after they have hair'd and grain'd the hide, they make a lather of the *mooſe*'s brains in warm water; and after they have ſoak'd the hide for ſome time, they ſtretch and ſupple it.

Some Remarks on the Allowances to be made in Aſtronomical Obſervations for the Refraction of the Air, *together with an accurate Table of Refractions; by Dr.* Halley. Phil. Tranſ. N° 368. p. 169.

WERE the medium of our air much more in quantity or the force of gravity much greater than it is; or in a word, were the refractive power of the air much more ſenſible than we find it, nothing could have been a greater impediment to diſcoveries in aſtronomy: For, all objects appearing by refraction higher than they really are, till ſuch time as the laws and quantity of that refraction had been aſcertain'd, it would have been impoſſible to have been ſecure of the true place of any celeſtial object. But as it falls out to be ſo little, that none but nice inſtruments can perceive its effects, it was not

not discover'd to be any at all, till *Bernard Walther*'s time, about the year 1500; nor brought to any sort of rule till *Tycho Brahe*; nor ascertained, till Sir *Isaac Newton* made the first accurate tables of it. The curve which a beam of light describes, as it approaches the earth, is one of the most perplext and intricate that can well be propos'd, as Dr. *Brook Taylor* has shewn in the last proposition of his *Methodus incrementorum.*

By this table it follows, that the ratio of the sine of the angle of incidence to that of the refracted angle, increasing as the beam approaches, makes a very notable difference in the true place of an object near the horizon; but in objects that are much elevated, the refractions become small, and their differences scarce exceed a second in each degree; so that they are much the same, as if the incident and refracted angles were on the surface of a sphere of air of the same uniform density, close adjoining to the eye.

When, therefore, the stars are 20 degrees or upwards elevated above the horizon, we may take it for granted, without sensible error, that the sines of the true and apparent distances from the vertex, are in the same constant ratio: Hence it will appear, that the distances of all the stars are seen less than they really are, in whatever position they are taken; and that no less than a second in each degree of the distance; that is, a distance of 30 degrees, for example, is contracted at least so many seconds; and one of 60 degrees no less than a minute, if the distances be taken by an instrument, truly divided: So that when *Hevelius*, to shew the exactness of his observations, brings eight distances, as taken by his sextant, which exactly compleat the circle, both in Long. and R. Ascens. the consequence is really quite opposite to his design; for, if those distances were the true ones, they being all contracted by appearing thro' a refracting medium, the sum of the eight differences both of Long. and R. Ascens. ought to fall short of a whole circle or 360 degrees by at least six minutes: So that Dr. *Halley* is apt to think, that the 60 degrees of *Hevelius*'s sextant wanted about a minute of its true quantity.

Such an allowance as this may, perhaps, be a proper expedient, to avoid accounting for refraction in celestial observations, provided the objects be nearly parallel to the horizon, or at a good height above it: For, all distances of stars, when parallel to the horizon, by the same constant quantity are con-

tracted by refraction, (be they high or low) that is, by about one ſecond in each degree; the chords of the arches of the real and viſible diſtances being always in the ſame ratio as is the ſine of the angle of incidence to that of the refracted angle.

And this is the caſe wherein the refraction of the air does leaſt affect the diſtances of the ſtars; which diſtances are ſtill more and more contracted, as they are nearer to a perpendicular ſituation: So that a diſtance, for example, of 30 degrees loſes but half a minute in a horizontal ſite; but if the one ſtar be 20 degrees high, and the other 50, it will be leſſened by above three times as much; or by 1 minute 41 ſeconds: If the one be 30 and the other 60 degrees high, the ſame diſtance will appear leſs than 30 degrees by about one minute; the difference ſtill decreaſing, as the objects are more elevated above the horizon: But in all caſes to account for the effect of the refraction on the diſtances of the ſtars, requires, beſides ſome trigonometrical calculation, the help of the above-mentioned table, hereto ſubjoin'd, and ſuch as Dr. *Halley* had it from its great author, Sir *Iſaac Newton*.

A Table

A Table of the Refractions of the ſtars for their apparent altitudes.

app. alti. deg.	min	refraction m.	ſec.	app. altit. degr.	refraction m.	ſec.	app. altit. degr.	refraction. m.	ſec.
0	0	33	45	16	3	4	46	0	52
0	15	30	24	17	2	53	47	0	50
0	30	27	35	18	2	43	48	0	48
0	45	25	11	19	2	34	49	0	47
1	0	23	7	20	2	26	50	0	45
1	15	21	20	21	2	18	51	0	44
1	30	19	46	22	2	11	52	0	42
1	45	18	22	23	2	5	53	0	40
2	0	17	8	24	1	59	54	0	39
2	30	15	2	25	1	54	55	0	38
3	0	13	20	26	1	49	56	0	36
3	30	11	57	27	1	44	57	0	35
4	0	10	48	28	1	40	58	0	34
4	30	9	50	29	1	36	59	0	32
5	0	9	2	30	1	32	60	0	31
5	30	8	21	31	1	28	61	0	30
6	0	7	45	32	1	25	62	0	28
6	30	7	14	33	1	22	63	0	27
7	0	6	47	34	1	19	64	0	26
7	30	6	22	35	1	16	65	0	25
8	0	6	0	36	1	13	66	0	24
8	30	5	40	37	1	11	67	0	23
9	0	5	22	38	1	8	68	0	22
9	30	5	6	39	1	6	69	0	21
10	0	4	52	40	1	4	70	0	20
11	0	4	27	41	1	2	71	0	19
12	0	4	5	42	1	0	72	0	18
13	0	3	47	43	0	58	73	0	17
14	0	3	31	44	0	56	74	0	16
15	0	3	17	45	0	54	75	0	15

The

The Variation of the Magnetical Compaſs *in the* Pacifick Ocean; *by Capt.* Rogers; *with Remarks thereon; by Dr.* Halley. Phil. Tranſ. N° 368. p. 173.

IN 1709-10 Capt. *Woods Rogers*, having travers'd the *Great South-ſea* or *Pacifick Ocean*, ſet down the variations of the magnetical compaſs in his paſſage from *Cape St. Lucar* in *Caleſornia* to the iſland of *Guam* or *Guana*, one of the *Ladrones*, being about ſeven hours or 105 degrees of Long. This might have been long ſince expected from Capt. *Dampier*, who had three ſeveral times made the tour of the world, and thrice gone this very ſame track.

It were to be wiſh'd, that the *French*, who have had frequent opportunities of doing it, would give us an account of the variations they have found in their voyages from *Peru* and *Chili* to *China*; and that the *Spaniards* would tell us, how the needle varies at this time, *viz.* 1721 in the north part of that great ſea, thro' which they return from the *Manilla's* to *New Spain*: With theſe helps, having three points in each curve, we might be enabled with tolerable certainty to compleat the ſyſtem of the magnetical variations, which Dr. *Halley* was oblig'd to leave unfiniſh'd, as to this part of the ocean, in his general chart thereof, for want of the requiſite obſervations.

The following account is an extract from Capt. *Roger's* journal; wherein the firſt column gives the correct Lat. of the place; the ſecond the Long. weſt from *London*, as eſtimated by reckoning; and the third the variation, which in this whole track is eaſterly.

Variations obſerv'd in the Great South-Sea, from the South Cape of Caleſornia *to the Iſland of* Guana *or* Guam, *one of the* Ladrones.

January $17\frac{0}{1}\frac{2}{0}$	Lat. N. cor. every Day.		Long. Weſt from *London*.		Variation Eaſterly.	
12	22	16	114	09	03	00
	21	18	114	42	02	50
	20	24	115	15	02	50
15	19	25	115	45	02	50
	18	56	116	24	02	45
			117	06	02	45
	18	0°	117	30	02	15

Vari-

Variations obſerv'd in the South-Sea,

January 1709-10,	Lat. N. correct.	daily	Long. Weſt from *London*.		Variation Eaſterly.	
15	16	32	118	05	02	00
20	15	44	118	54	01	50
	15	00	120	15	01	30
22	14	49	122	05	01	10
	14	36	124	25	00	50
	14	24	126	45	00	40
25	14	14	129	05	00	45
	13	50	131	23	00	50
	13	29	132	58	01	00
	13	29	134	41	01	10
	13	22	136	48	01	15
30	13	27	139	21	01	25
	13	32	142	07	01	30
Feb. 1	13	32	144	37	01	40
	13	36	147	32	01	50
	13	26	150	18	02	00
5	13	26	153	02	02	10
	13	26	155	19	02	25
	13	26	157	43	02	30
	13	25	160	31	02	50
	13	41	163	00	03	00
	13	41	165	18	03	20
10	13	44	167	26	03	30
	13	36	169	56	03	45
	13	33	172	27	04	00
	13	36	175	00	04	30
	13	32	177	21	05	20
15	13	40	179	28	06	30
	13	47	181	24	07	00
	13	54	183	22	07	30
	13	52	185	37	09	00
	13	40	187	42	10	15
20	13	28	189	49	11	00
	13	21	191	30	11	30
	13	12	193	25	12	00
	13	07	194	37	11	50
	13	10	195	51	11	00
25	13	03	197	51	10	00
	13	00	199	03	09	50
	12	57	200	16	09	30
	12	54	202	20	09	00
March 1	12	58	204	12	08	40
	13	04	206	06	08	20

Vari-

Variations obſerv'd in the South-Sea.

1709-10.	Lat. N. correct. daily	Long. Weſt from *London*.	Variation Eaſterly.
March 3	13 05	207 33	08 00
	13 05	209 04	07 50
5	13 02	211 54	07 30
	13 07	212 42	07 10
	13 07	214 07	07 00
	13 03	215 08	06 50
	13 08	217 11	06 30
10	13 16	218 27	05 40
	Iſland of *Guana* in ſight.		

By this it appears; that at about 250 or 300 leagues weſt from the ſouth cape of *Caleſornia*, the eaſt variation diminiſhes to about $\frac{3}{4}$ of a degree; that for 1300 leagues from thence, the ſame eaſterly variation gradually increaſes to about 12 degrees, where it becomes greateſt; and that at the iſle of *Guam*, 500 leagues ſtill more weſterly, it is again decreas'd to five degrees 40 minutes.

As far as this ſingle inſtance can direct us, the Dr. is apt to think, that in all that ſpace of ſea, lying to the northward of the track, between *Japon* and *Caleſornia*, there prevails an eaſterly variation, which is ſtill greater and greater, as the N. Lat. increaſes: But that to the ſouthward of the track and eſpecially to the ſouthward of the equinoctial, a weſterly variation ariſes, of no great extent or quantity, but which is greateſt about 1000 leagues weſt from the coaſts of *Peru* and *Chili*, about the ſame meridians where Capt. *Rogers* found the eaſt variation ſmalleſt.

This is agreeable to the theory of the variation laid down by the Dr. in *Phil. Tranſ.* N° 148; and in his ſeventh remark on the obſervations there cited, he expreſsly mentions, that, undoubtedly there was ſuch a track of weſt variation in the ſouthern parts of the ſouth-ſea; it being the neceſſary conſequence of the ſite of the four magnetical poles, there ſuppos'd; tho' at that time he wanted experiments to prove it.

A far-

A farther Account of the Art of Diving *or living under* Water; *by the Same.* Phil. Tranſ. N° 368. p. 177.

IN *Phil. Tranſ.* N° 349. Dr. *Halley* ſufficiently explained the method he had practiſed and found effectual to furniſh air at any reaſonable depth under water, and in any quantity deſired, for the ſubſiſtence of men, that ſhall have occaſion to work on wrecks or otherwiſe at the bottom, under a great preſſure of water. This the Dr. did by means of the diving-bell, which being from time to time repleniſhed with freſh air, he had found ſufficient to maintain five men for near two hours together in 10 fathom water, without the leaſt hurt or inconveniency. But the bell being not to be moved from place to place, but by moving the veſſel from which it was ſuſpended, was a great impediment to the work that was to be done below; and therefore, the Dr. bethought himſelf how to enable the diver to go out from the bell to a conſiderable diſtance and to ſtay a ſufficient time without it, with full freedom to act as occaſion ſerved: And conſidering, that the preſſure being greater on the ſurface of the water in the bell, than on any other ſurface that was higher than it, the air would by a pipe paſs from the bell into a cavity of air over that higher ſurface; he concluded, that putting on a cap of lead made weighty enough to ſink empty, and in ſhape reſembling the bell itſelf, he might, by flexible pipes, which a man might carry coiled on his arm, receive a conſtant ſtream of air from the magazine thereof in the large bell, ſo long as the ſurface of the water in the caps was above the level of that in the bell.

In purſuance of this, the Dr. procured pipes to be made, which anſwered all that was expected from them: They were ſecured againſt the preſſure of the water by a ſpiral braſs wire, which kept them open from end to end, the diameter of the cavity being about the ſixth part of an inch: Theſe wires were coated with thin glove-leather, curiouſly ſewed on and then the leather was dipped into a mixture of oil and bees-wax hot, which filling up its pores, made it impenetrable to water; then ſeveral folds of ſheeps guts were drawn over them, which when dry, had a good coat of paint; and then the whole was ſecured with another coat of leather to keep them from fretting: The pipes (of which ſeveral were made were about 40 foot long, the ſize of a half inch rope; the one end thereof being fixt in the bell, at ſome height above the water, and the other end faſtened to a cock, which opened into the cap; the uſe of the cock being to ſtop the return of the air, whenever there was occaſion to ſtoop

down, or go below the ſurface of the air in the bell, which was neceſſary, as often as there was occaſion of going out or returning into the bell.

The diver, therefore, putting on his cap, and coiling his pipe, (like a rope) on his arm, as ſoon as he is diſcharged from the bell, opens his cock and walks on the bottom of the ſea, veering out the coils of his pipe, that ſerve as a clue to direct him back again; and this the Dr. has ſeen practiſed, without any bad conſequence attending it.

But there are two things to be remarked in this affair; firſt, that the weight of a man being very little more than that of his bulk in water, he cannot act with any ſtrength, nor ſtand with any firmneſs; eſpecially, where any thing of a ſtream runs, without a conſiderable addition of weight; and, therefore, the leaden caps were made to weigh about half a hundred weight, to which he added a girdle of large weights of lead (of about the ſame weight in the whole) to be wore about the waiſt; and two clogs of lead for the feet, of about 12 pounds each: With this acceſſion of weight the Dr. found a man could ſtand well in an ordinary ſtream and even go againſt it. The other thing neceſſary to be provided againſt was the cold of the water, which tho' it could not be entirely taken off, ſo that a man could endure it long, yet it was much abated by waiſtcoats and drawers, made cloſe to the body, of that thick ſort of woollen ſtuff whereof blankets are made: This being full of water, would be a little warmed by the heat of the body, and keep off the chill of new cold water coming on it.

As to ſeeing under water; as long as the water is not turbid, things are ſeen ſufficiently diſtinct; but a ſmall degree of thickneſs makes perfect night, in no great depth of water.

In the leaden caps, which from their uſe the Dr. called *caps of maintenance*, he at firſt fixed a plain glaſs before the ſight; but he ſoon found, that the vapour of the breath would form ſuch a dew on the ſurface of the glaſs, that it hindered its tranſparency.

To remedy which, he found it neceſſary to prolong that ſide of the cap that was before the eyes, and thereby enlarge the proſpect of what was beneath.

An Account of an Aurora Borealis, *obſerved at* Dublin. Phil. Tranſ. N° 368. p. 180.

AT *Dublin Feb.* 6. 1720-1 the obſervations made on an *aurora borealis* are, as follows; the air was all that day, as it had been for ſome time before, very clear and ſharp; about half an

an hour paſt four in the evening, ſome flying clouds appeared, and the ſky was tinged with a very unuſual yellowiſh colour, which, perhaps, might be reflected from a large quantity of ſnow, that ſoon after fell for near a quarter of an hour: However that might be, the author dates the beginning of the enſuing phænomena from the firſt appearance of this uncommon light: About a quarter paſt ſix, a thin vapour, which was hitherto ill defined, and in all appearance reſembled an exceeding black cloud, had fixed itſelf in the northern hemiſphere; its edges were tinged with a reddiſh yellow, that by degrees, as it approached the vertex, became more diluted, till at laſt it ended in a faint whiteneſs: That it really was no cloud, but only an exceeding pure and limpid vapour, was manifeſt, becauſe ſeveral of the fixed ſtars ſhone thro' it, without having their light in any degree effaced: In the middle of this dark baſis, about ½ an hour paſt ſix, a lucid area ſhewed itſelf due N. E. about 35 degrees above the horizon; and in leſs than a minute from the time he firſt diſcovered it, emitted a very large pyramidal ſtream of ſhining vapour, which with an incredible ſwiftneſs aſcended obliquely towards S. S. W; ſo as to leave the zenith conſiderably to the weſtward; and very ſoon after, about the ſame place, ſix others aroſe at the ſame inſtant almoſt up to the zenith. From this time till 48 minutes paſt ſix, there were repeated projections of theſe lucid rays, without any order as to time, place or magnitude. They did not only ariſe from behind the dark baſis, but ſometimes out of the pure ſky, as it were; and tho' ſome of them continued viſible upwards of a minute, yet the greater part of them only juſt ſhewed themſelves and died away. The author had now got to the top of a convenient obſervatory, where, tho' deſtitute of inſtruments, he had a free proſpect of the horizon and in company with another Gentleman, fixed himſelf with great attention, to expect the enſuing phaſes of this phænomenon.

About ſix hours 55 minutes between N. W. by N. and W. N. N. there was the repreſentation of a very bright *crepuſculum*, ſuch as that, which appears about 20 minutes after ſun-ſet; and from it aroſe ſeveral very large beams of light, not exactly erect towards the vertex, but ſomewhat declining to the ſouth: Among theſe one, which aroſe about N. W. and in three or four ſeconds of time paſſed over 50 or 60 degrees of a great circle, was the moſt ſplendid of all that had preceeded; its ſides were inclined to each other in an angle of about 8 or 10 degrees and tinged with a briſk lively red, which by degrees, as it approached the

axis, became more intenſe and dirty: On the other hand receding from the axis, its colour was a pale yellow, that ſoon loſt itſelf in a faint whiteneſs.

From this time not a moment paſſed without ſuch variety of different phaſes, that it was impoſſible for the eye of any ſingle perſon to purſue it thro' the ſuddenneſs of its alteration. While ſome of the lucid beams ſeemed to ſtand fixed, as it were, among the ſtars, others moved ſlowly from eaſt to weſt, by which they ſeemed to meet each other, ſometimes to recede from each other; and ſometimes by a kind of appoſition, large ones were produced from others of an inferior order.

The lucid area, which the author firſt diſcovered in the N. E. had now formed itſelf into a parallelogram, whoſe upper and lower edges were five or ſix degrees diſtant from each other, and nearly parallel to the horizon; in this, as if behind a curtain, vaſt waves of light, whoſe extremities did not reach the periphery of the dark baſis, ſeemed to meet and pervade each other; at other times, while ſome of them, with a remarkable velocity, moved eaſtwards; others, as if behind them, would fly towards the weſt; by which variety of different motions, as often as any interval paſs'd between the colliſion of theſe erect waves a beautiful undulation was produced; and its pulſes, by the adjoining parts of the fluid, were propagated to a vaſt diſtance.

While he ſtood amazed at this ſurpriſing ſight, the axis of the coloured pyramid, which aroſe in the N. W. had moved conſiderably towards the weſt, and at ſeven hours 25 minutes was about 23 or 25 degrees to the north of *Venus*. The dark baſis of this meteor had now extended almoſt to the eaſt point of the horizon; and at ½ an hour paſt 7, between E. N. E. and E. by N. ſeveral large columns aſcended in an inſtant to the zenith; the moſt eaſtward of which was remarkably convex towards the ſouth, and tinged with a pale red, as were moſt of thoſe that aſcended with it: They were met by others, that aroſe at the ſame time between the north and weſt, and in the zenith formed a vaſt collection of vapour, that pretty much reſembled ſmoke, enlightened by the ſun's beams; its waves reflected a briſk, lively red colour and in ſome places a pale yellow; they rolled indifferently every way; and in little more than a minute, when the firſt efforts of their congreſs were ſpent, and all ſeemed fixed and ſerene, the *corona* projected ſeveral ſmall rays, which, with a ſlow uniform velocity, deſcended between W. by N. and N. W. ſoon after which it died away.

At

At 7 hours 40 minutes ſeveral other *ſtriæ* were emitted from behind the dark baſis, which interſecting with others, that at the ſame time aroſe about the eaſt and weſt points, formed in the zenith, or rather 6 or 8 degrees to the ſouth thereof, a ſecond much more elegant and ſurpriſing than the former, and, indeed, than any thing had hitherto appeared; it was tinged not only with different orders of red and yellow, but alſo with blue and violet; the laſt of which by a mixture with the white light, appeared faint and inclined to purple. Tho' the vapour of which this and the preceeding *corona* were formed, was ſo exceeding thin and pure, that ſeveral of the fixed ſtars were viſible thro' it; yet it reflected a light ſo copious, that he could thereby perfectly diſtinguiſh the time of night by a ſmall watch: All this while, the phænomenon ejected four or five large columns towards the N. W. (beſides others towards the ſouth) which appeared pointed at the top, and their ſides inclined to each other in an angle of five or ſix degrees: When their baſes were extended about 30 or 35 degrees from the vertex, the lower parts of two or three of them broke, by the mere weight of the vapour (as it were) ſeparated from the upper and deſcended with a ſlow motion, in the form of truncated cones; they were gradually formed by their upper parts, and in about a minute's time loſt in a large body of light, that was ſettled between the N. W. by N. and W. N. W. The *corona*, as if exhauſted by theſe great diſcharges, became immediately more dilute and languid; its lively colours faded and were ſucceeded by a whitiſh vibrating light, that in leſs than two minutes entirely vaniſhed.

The dark vapour, which continued to poſſeſs the polar regions, had now extended itſelf from the E. to the N. W. by N. point of the horizon, and was formed into a large ſegment of a circle, whoſe center was about 20 degrees below the horizon; its upper edge was tinged with a pale red, which was ſoon loſt in a florid yellow; and this again, as it approached the zenith, became more effoete and languid: In this dark ſegment ſeveral lucid area's diſcovered themſelves, with a vibrating light, which inſtantly diſappeared, as if a curtain were drawn over them; and from it rays of very different magnitudes continued to aſcend without any uniformity as to time and place, till 48 or 49 min. after 7; when a third *corona*, very little, if at all, inferior to the preceeding ones, either in the variety of its colours, or the quantity of light it emitted, was formed in the zenith. As both the preceeding were produced by the northern *ſtriæ*; ſo this was augmented by two or three large ones, that aroſe due ſouth, out

of

of the pure ſky, and were, in all probability, part of the vapour, which had been projected beyond the zenith, or which had ſubſided from the two former: They cauſed the vapour of which this image was compoſed, to move with great violence, in different directions, not unlike waves of ſmoke, confined in a reverberating furnace; this motion being abated, the vapour acquired a kind of ſtagnation, in which ſtate it continued but a very ſhort time, before it projected ſeveral lucid beams (a certain fore-runner of its approaching diſſolution) between the N. and W. and ſoon after it vaniſhed.

About this time the large beam, which aroſe in the N. W. and had preſerved its colours in their original beauty, for more than ¾ of an hour, began to fade, and at 7^h 53′ was abſorbed in a vaſt body of light, which ſeemed fixed in that part of the horizon; it had moved in that time 15 or 20 degrees to the weſtward of the place from whence it aroſe: The *impetus* of the vapour being now pretty much abated, there was nothing extraordinary, but ſucceſſive diſcharges of pointed rays between the N. W. and E. N. E. without any order or uniformity as to time or place; ſetting aſide theſe, there was very little difference in the general face of things for 20 minutes; neither was there much reaſon to hope for any, becauſe the ſeverity of the cold was ſuch as obliged the ſpectators to retire and ſo loſe the enſuing phaſes of the dying meteor.

An Account of an Aurora Borealis, *obſerved on the ſame Day at* Cruwys-Morehard *in* Devonſhire, *as alſo of the Weather both before and after this* Phænomenon; *by Mr.* Cruwys. Phil. Tranſ. N° 368. p. 186.

ON *Monday Feb.* 6. 1720-21 a little before ſeven in the evening, there aroſe out of the north or a little towards the eaſt, a bright *crepuſculum*, which ſoon ſpread itſelf a great way thro' the northern part of the hemiſphere. About ſeven (when Mr. *Cruwys* firſt obſerved it) it began to leave behind it at due north or a few degrees to the eaſt, part of a very clear ſky (which looked like a black cloud, but the ſtars ſhone clearly thro' it) being a ſegment of a circle, into which figure the *crepuſculum* or expanded body of lucid vapour had now likewiſe formed its upper limb, making a kind of broad *iris*, terminated at each end by the horizon.

All this while the ſtreaming lights appeared in great variety, as to figure, place, magnitude and colour; but for the moſt part of a redder colour, eſpecially towards the weſt, than the *crepuſculum*

culum itſelf, out of which they ſeemed to be formed; and tho' the greateſt appearances had been for the moſt part within 20 degrees of the north on each ſide, yet at due N. W. there were a great many conſiderable ones.

About eight this *crepuſculum* (which had been conſtantly, tho' ſlowly, carried farther from the north) had, with the upper part of its outer limb, reached to about 10 or 12 degrees beyond the pole-ſtar towards the zenith, being now upwards of 30 degrees broad, with a circular ſegment of black clear ſky to the north, of about 25 degrees, when the whole *crepuſculum* or vapour was all of a ſudden formed into aggregate bodies like vapours, and was one of the moſt agreeable ſights, that had been obſerved of the kind: The baſes of the cones ſeemed to reſt on the upper limb of the ſegment of clear ſky (which was extended near 60 degrees on each ſide the N.) and the vertices of the cones, pointing all towards the zenith, approached within a few degrees of it and terminating there, formed the greateſt part of a ſemicircle, incloſed, as it were, with golden palliſadoes, which ſhining all at once almoſt as bright as flame, and being of a vaſt length and number, exhibited a very agreeable ſight.

This laſt phænomenon ſhewed, that theſe cones were collections of the very ſame particles, of which the *crepuſculum* had conſiſted; becauſe when it appeared every where alike and equal, the great ſtars ſhined thro' it but very faintly: Whereas afterwards, thoſe ſtars, that remained between the cones, ſuddenly appeared very bright; whilſt thoſe, that were covered by them, could hardly be perceived: And, indeed, all the ſtreaming lights this evening ſeemed to flow from this *crepuſculum* downwards, as from a fountain or ſtore, and not to ariſe from the horizon; few approaching it nearer than 10 degrees, and many not within 20 or 30 degrees.

After this fine appearance had continued about two minutes, the matter ſeemed to be exhauſted and the ſcene almoſt at an end, the ſtreaming ſhining lights being moſtly extinguiſhed and the remaining parts of vapour, reſembling broken clouds; when the flaſhing lights began to appear of a moſt prodigious ſwiftneſs, both from N. E. and N. W. pointing to the zenith or a little more to the ſouth: It was obſervable, that over the tracts where theſe flaſhing lights paſſed, the remaining parts of vapour (that now lay ſcattered every where, like white broken clouds) pointed or ſeemed to have a tendency, conformable to the ſame motion: Whereas towards the due north, where no flaſhes appeared, theſe whitiſh clouds lay confuſed and irregular as before.

This

This continued about 20 or 25 minutes, when the wind began to ariſe a little at N. E. and the ſcene was quite at an end; dark clouds ſucceeding all over the N. and by 9 o'clock there was a ſevere ſtorm of ſnow.

N. B. That all the time of theſe appearances, ſeveral broken parts of the extinguiſhed vapours, reſembling white clouds, were carried beyond the zenith; ſome 50 or 60 degrees, and others even to the horizon itſelf at S. S. W.

During the whole continuance, there ſeemed to be a ſmall, eaſy breath of wind, ſcarce perceivable, at N. N. E. which the motion of the clouds above-mentioned alſo confirms; but as ſoon as it began to blow a little briſker, the remaining parts of the vapour were all diſſipated.

January 30. 1720-1. Hard ſnow in the morning and froſt all night.

January 31. and *February* 1, 2, 3, 4. Pleaſant ſun-ſhine days, very calm, but the ſnow ſtill lying and at nights very hard froſts.

Feb. 5. Very violent ſnow in the morning, ſome thaw in the afternoon and hard froſt at night.

6. Hard froſt in the morning and the wind exceeding cold and ſharp, but not hard; the afternoon pleaſant, ſun-ſhine and calm; but it froze all day out of the ſun; and continued to do ſo all the evening, and the ground was ſtill covered with ſnow.

7. Very hard froſt in the morning, and froſt and ſtorms of ſnow all day; the wind at N. N. W.

8, 9, 10, 11. Pleaſant calm days; but hard froſt and very hard froſt at night.

12. Hard froſt; exceeding cold wind at S. E. at night theſe lights were ſaid to have appeared again; as alſo on the 18. to a very great degree, but Mr. *Cruwys* did not ſee them; the weather ſtill froſty with ſome ſnow.

Feb. 22. At night hard ſnow.

23, 27. A thaw and ſome rain and but little froſt afterwards, only dry cold winds, till the 27. when the froſt returned very ſharp, with exceeding cold winds at N. E. and S. E. for a fortnight and upwards, without any ſnow, and did a great deal of damage.

Obſer-

Obſervations on the Muſcular Fibres *of* Fiſh; *by M.* Leewenhoeck. Phil. Tranſ. N° 368. p. 190.

THE aſſertion, that nature in all her various productions conſtantly obſerves the ſame courſe and manner of operation ſeems contrary to M. *Leewenhoeck*'s obſervations; either thoſe made on the generation of animals and the ſeeds of plants; or even thoſe on the muſcles and muſcular fibres of different animals; foraſmuch as the muſcles of fiſhes are not provided with any tendons: On this occaſion M. *Leewenhoeck* examined anew the muſcles of the cod-fiſh, on which he made the following obſervations.

After the late diſcoveries he had made of the ſmall veſſels in the muſcular fibres of a whale, ox, ſheep and mouſe, he was apt to imagine, that the ſame fabric would likewiſe hold in the muſcular fibres of fiſh: But as this could not be certainly concluded, having at that time a part of a freſh cod, he cut off a piece of the fiſh, on purpoſe to examine it with his microſcope ſome days afterwards.

He cut this piece of fiſh into ſmall ſlices, ſome according to the length of the fibres and others directly a-croſs, in order to obſerve, whether theſe muſcular fibres were compoſed of great numbers of ſmall veſſels, running along the fibre: And in effect he found, that when he had cut the fibres thro', there appeared in the microſcope as great a number of ſmall veſſels running along theſe fibres, as he had formerly obſerved in the muſcular fibres of a whale.

But what appeared moſt remarkable was, that in a great number of fibres, in which he could not diſcover any veſſels running lengthways, he obſerved a vaſt many ſmall veſſels, which ſeem'd to proceed from the membranes encompaſſing the fibres: For, in one fibre theſe veſſels appeared to proceed from the circumference or circular tunicle of the fibre and to paſs on to the oppoſite part thereof: And in another fibre cut tranſverſely, he obſerved veſſels ariſing from the circumference and dividing themſelves into ſmaller branches about the middle of the fibre; all which, as far as he could perceive, terminated again in the circumference of the fibre: In one fibre he obſerved at leaſt fifty of theſe veſſels, running thro' each other.

Upon this diſcovery he found, that he had been miſtaken in what he had firſt imagined, to wit, that the veſſels which aroſe from the membranes, proceeded no farther than juſt thro'

the tunicle of the fibre, and ſo diſcharged the fluid into the fibre for its nouriſhment: Whereas now he perceived, that the veſſels which aroſe from the membrane and enter'd into the fibre, did not end there, but ſpread themſelves into ſmaller branches, proceeding every way from the inſide to the tunicle of the fibre. This made M. *Leewenhoeck* imagine, that the nutritious juice might circulate in theſe ſmall veſſels, juſt as the blood does in the veins and arteries; and that what the muſcular fibres receiv'd from them, might be no more than what ouz'd thro' the tunicles of theſe ſmall veſſels, as in land-animals, which have no other termination than the artery proceeding from the heart and the vein terminating therein; the artery and vein thus forming one continued veſſel,

Having now a great many fibres lying before him, in which he could obſerve very plainly the veſſels juſt now treated of, yet he could not diſcern in the tranſverſe ſections of the fibres any appearance of thoſe veſſels, which run along their length and compoſe the greateſt part of the body of each fibre: this he imputed to the cutting of thoſe veſſels not directly acroſs, but ſomewhat obliquely, by which their apertures had been clos'd in ſuch a manner, that he could not perceive them, nor the leaſt reſemblance of them.

M. *Leewenhoeck* ſeveral times obſerv'd between the muſcular fibres of the fiſh, a great many veſſels lying together, which compos'd what is call'd a membrane; which veſſels ſurrounded the muſcular fibres, and lay ſo many of them together, that the thickneſs of the whole *faſciculus* of veſſels was equal to that of a muſcular fibre, and as he imagin'd, was afterwards to be diſpers'd in ſmaller ramifications between the fibres.

In viewing an entire muſcle of a cod-fiſh, and the fibres of which it was compos'd, he found the thick end of the muſcle equal to the back of an ordinary knife, and the thinner not to exceed the thickneſs of a ſingle fibre: Several of theſe fibres are twice as long as the thickneſs of the muſcle; and between the muſcles lie what are commonly call'd membranes, which are nothing elſe but a congeries of veſſels. Theſe veſſels do not only run between the fibres, but into the very ſubſtance of every fibre, as we obſerve, when the fibres are cut tranſverſely: By theſe veſſels the muſcular fibres and the entire muſcles themſelves are ſo firmly bound together, that they ſerve inſtead of tendons to each other.

In

In like manner the muſcular fibres are united to the bones by the veſſels proceeding from them, which veſſels compoſe what in land animals is call'd the *perioſteum.*

A B C D Fig. 7. Plate IV. repreſents two muſcles of a cod-fiſh, lying cloſe together, as they are united to each other and ſeparated from the other muſcles; the part repreſented by A B C having been cover'd with the ſkin near the head of the fiſh; and M. *Leewenhoeck* ſuppoſes, that the body of the cod-fiſh, from head to tail, conſiſts of a continued ſeries of ſuch muſcles.

E F G H Fig. 8. repreſents a ſingle muſcle of the fiſh; where E H G ſhews the thickneſs of the muſcle; and its thin edge, which is no thicker than the edge of a knife is mark'd by E F G.

When theſe muſcles had lain ſeveral days upon a paper, they were not dried ſo hard, but that he could ſplit them into thin ſhivers, one of which is repreſented between the letters I and K Fig. 9. in order to ſhew the oblique courſe of the fibres, which are repreſented by ſmall lines.

M. *Leewenhoeck* turn'd his thoughts to river fiſh and particularly to pearch; and as he imagin'd, that an old pearch had no greater number of muſcular fibres than a young one, but only that the fibres increas'd in bigneſs during the growth of the fiſh; and that the larger theſe fibres were, the more plain and diſtinct muſt be the ſmall veſſels, of which the fibres were compos'd, he procured one that weighed three pounds and half, and was 17 inches and ½ long *Delft* meaſure, which is the ſame with the *Rhinland.*

He cut off four pieces from this fiſh, *viz.* two from the back near the head, and two others from the belly in the thick part of the fiſh, on purpoſe to make his obſervations thereon next day.

Accordingly at that time he view'd the muſcular fibres both as to length and breadth, and found, that the fibres of this large pearch were not ſo thick as thoſe of the cod-fiſh: Upon cutting them thro' lengthways, he obſerv'd the apertures of the ſmall veſſels in very great numbers: He next cut ſome of the fibres tranſverſely, and he plainly found them thinner in this pearch than in a middling cod-fiſh, and he obſerv'd the ſmall veſſels, that compoſe the greateſt part of the bulk of the fibre, lying as cloſe together, as ever he obſerv'd them in any kind of fiſh or fleſh.

L M N O Fig. 10 repreſents a ſmall portion of the muſcular fibres of the fiſh, cut thro' tranſverſely, after they were grown dry, and in their ſhrinking had been torn off from the ſmall veſſels that incompaſs'd them; the openings of the ſmall veſſels in theſe fibres were diſtinctly to be ſeen, but they appeared in ſuch great numbers and were ſo exceedingly ſmall, that it was impoſſible to repreſent them any otherways than by points.

In this figure are repreſented what we call the membranes, but which indeed are nothing elſe than a congeries of ſmall veſſels, which not only ſurround the fibres, but enter into their very ſubſtance. Theſe in the ſhrinking and drying of the object upon the glaſs-plate had been torn off from the fibres, as may be ſeen at P P P.

When this was done, M. *Leewenhoeck* put a ſmall drop of water, about the ſize of a pin's head, on this ſmall portion of fibres, into which it immediately inſinuated and ſwell'd them to the ſame bigneſs, as when they were firſt laid upon the plate: After which the deſigner drew them, as they then appeared to him, omitting to repreſent the ſmall veſſels and only deſigning the circumference of every fibre, as appears at Q R S T, Fig. 11.

M. *Leewenhoeck* then ſplit a grain of millet thro' the middle, and placing one half of it upon the glaſs, hard by the portion of fibres, repreſented in Fig. 10. the deſigner obſerv'd, that the half of the grain of millet-ſeed as repreſented by Fig. 12. appeared larger than the portion of fibres; as did likewiſe another perſon who view'd them: By which one may eaſily imagine, in how ſmall a ſpace that number of fibres is comprehended, each of which conſiſts of ſo many veſſels.

He likewiſe made his obſervations on the muſcular fibres of a pike, roach, ſcar and flounder; in each of which he found the fibres compos'd of ſmall veſſels, like thoſe of a cod and pearch.

He took the largeſt dried ſprat he could procure, being a little more than five inches long, and he found that the fibres of a ſprat were but little thinner than thoſe of the large pearch above-mentioned; and that the veſſels of which the fibres were compos'd, were nearly as numerous, as in the fibres of the pearch.

From theſe obſervations ſome may be apt to conclude, that the muſcular fibres of land-animals are of the ſame thickneſs with thoſe of fiſh: But for the ſatisfaction of ſuch as have not

ſeen

ſeen the objects here ſpoken of, he caus'd a ſmall portion of the muſcular fibres of a large ox to be delineated, as they appear'd thro' the ſame microſcope with the former, to ſhew the thickneſs of the dried fibres, and the veſſels which compoſe them, as is repreſented by XYZ Fig. 13.

The deſigner obſerv'd in the tranſverſe ſection of one of theſe fibres, 25 veſſels.

Upon cutting tranſverſely ſome of the muſcular fibres of a ſmall ſmelt, of the length of about two joints of his finger, he placed them before a microſcope, and obſerv'd that theſe fibres were not only twice as thick as thoſe of an ox, but likewiſe provided with as great a number of veſſels, as the fibres of other fiſh.

Upon thus obſerving that the muſcular fibres of fiſh were much larger than thoſe of land-animals, he caſt about, for the reaſons of this diſproportion; but all the ſatisfaction he could meet with was, that as fiſh ſwim in the water, their muſcular fibres need to exert very little force, in order to ſupport their bodies therein, becauſe they are very nearly of the ſame ſpecific gravity with the element in which they ſwim; all the force they exert is in their progreſſive motion, in purſuit of their food: Whereas the muſcular fibres of land-animals exert a conſiderable force, not only in ſupporting and moving their bodies, but in carrying burdens and in other labour they are put to: And we muſt allow, that the ſmaller and finer the fibres are, to make a body of any determinate thickneſs, the ſtronger will be the compoſition; and therefore, the muſcles in fleſh muſt be ſtronger than thoſe of fiſh.

The wonderful and amazing ſtructure of theſe minute parts in the muſcles of beaſts and fiſh, which never enter'd into our thoughts, juſtly claims our admiration; and doubtleſs, there are ſeveral other ſurpriſing things in them, which will, perhaps, for ever eſcape our penetration.

M. *Leewenhoeck*, taking out a little of the mealy ſubſtance of a boil'd grey pea, laid it before a microſcope, where it appear'd to conſiſt of ſuch particles as are found in rats dung; every one of which parts conſiſted of a great number of very ſmall particles: But he could not diſcover any membranes enveloping thoſe parts; whence he concluded, all theſe membranes were deſtroy'd and diſſolv'd by the hot water.

Upon this, he took another grey pea, which had not been boil'd, and he cut it into very thin ſlices; when he not only obſerv'd the membranes, in which the parts of the mealy

ſub-

ſubſtance had been inclos'd; but likewiſe found, that thoſe membranes conſiſted of nothing elſe but a great number of very ſmall veſſels like the membranes (as they are commonly call'd) which ſurround the muſcles and muſcular fibres in beaſts and fiſh.

Obſervations on the Seeds of Plants; by the Same. Phil. Tranſ. N° 368. p. 200.

M. *Leewenhoeck* having often turn'd his thoughts to obſerve the membranes in which the ſubſtance of meal or flower is inclos'd, like little packets in cells or boxes, which is alſo the caſe of all kinds of beans, peaſe, wheat, barley and other grain; he at length with aſtoniſhment diſcover'd very plainly, that what he calls the membranes were endu'd with an unſpeakable number of little holes, thro' which, in ſeveral places, one might perceive the light; which holes, we muſt ſuppoſe to be nothing elſe but ſmall veſſels, torn or cut off; and which partly compoſe the membranes he calls little cells, and partly ſerve for the production of the *farina*, of which there are vaſt numbers of particles in a pea or bean; each of which, as ſmall as they are, he ſuppoſes, receives its increaſe from a ſmall veſſel, which proceeds from the aforeſaid cell, and is imperceptible by reaſon of its ſmallneſs.

Theſe veſſels, of which the little cells or caſes do moſtly conſiſt, are more eaſy to be diſcovered in beans and peaſe, than in any other ſort of pulſe or grain, but in wheat the veſſels are with difficulty traced in the cells; and M. *Leewenhoeck* has been oblig'd to make a great many obſervations and experiments before he could fully ſatisfy himſelf, that he ſaw the torn or broken veſſels; the reaſon of which is, that the ſmall veſſels of which the cells or ſkins of the grains of wheat are compos'd, are exceeding thin and brittle.

Moreover, he found, upon obſerving the veſſels, of which the cells are compos'd, that ſeveral of the globules in wheat were broken in pieces in the operation; and that in one of thoſe ſingle globules there were other ſmall globules inclos'd.

He likewiſe obſerv'd, that the membranes or little cells in barley, in which the globules or parcels of the meal are inclos'd and receiv'd their nouriſhment, are thicker and ſtronger than thoſe of the wheat.

Tho' M. *Leewenhoeck* concludes, that almoſt all ſeeds and grain, as well as their membranes or ſkins, are of one and the

ſame

ſame texture and configuration; yet for experiment ſake, he took a large almond, and cutting off ſeveral thin ſlices therefrom, he took out, as much as poſſible, the ſubſtance that lay in the little cells; and viewing them as nicely as poſſible with a microſcope, he obſerv'd, that thoſe cells, in which the oil of the ſaid almond was for the moſt part contain'd, did alſo conſiſt of nothing but ſmall veſſels.

He intended, had it been practicable, to view the ſmaller ſorts of ſeeds, in order to find out, whether the little cells, in which the farinaceous matter lay, were likewiſe compos'd of ſmall veſſels; but he changed his mind, imagining that his attempts therein would be ineffectual, by reaſon of the ſmallneſs of thoſe little cells; and conſequently of the veſſels, of which thoſe cells are compos'd; tho' he does not queſtion, but what we diſcover in the larger ſeeds is analogous to the ſtructure of the ſmall ones.

Now when he perceived that the above-mentioned little cells proceeded from the bark or ſkin, which ſurrounds the kernel of the ſeed or grain, he was thinking, that as the mealy ſubſtance receives its increaſe from the veſſels, which are in the little cells; and as the plant is form'd between the cells, during the time that the ſeed lies in the earth; and as the little orifices in the ſkin of animals and fruit, are form'd in order to diſcharge thereby the ſuperfluity of their moiſture, and ſhut in ſuch a manner, that no moiſture or common air can get into the ſame, as he formerly affirm'd; ſo on the contrary, the orifices of ſeeds are form'd in ſuch a manner, that ſeveral of their ſmall veſſels admit moiſture to paſs inwards, and accordingly water is driven into them by the preſſure of the air, which cauſes the ſeed to ſwell: Upon which a warmth and fermentation ſucceeding in the ſeed, it requires a larger ſpace; and by the particular formation of the particles, which lie in the cells, and have deriv'd their increaſe therefrom, the mealy ſubſtance of which they conſiſt, is partly driven out of them into the body of the young plant; which by this means increaſes ſo much in bulk, that the root can now ſupply it with nouriſhment from the earth; at which time the ſeed is found to be diminiſh'd in its bulk.

An Account of ſome Experiments relating to Magnetiſm; *by Dr.* Brook Taylor. Phil. Tranſ. N° 368. p. 204.

DR *Taylor*, after having given an account of an experiment made with a large magnet in the Repoſitory of the Royal Society (which experiment is deſcrib'd in *Phil. Tranſ.* N° 344) goes on with the ſame ſubject, as follows.

If it were known what point within the ſtone, and what point in the needle are the centers of the magnetical force, it would be eaſy to find the true forces of the magnet at all the diſtances obſerv'd: For want of that knowledge, the Dr. has computed the forces from the center of the needle and the extremity of the loadſtone; and finds, that at the diſtance of nine foot, the force alters faſter than as the cubes of the diſtances; whereas at the diſtances of one and two foot, the force alters nearly as the ſquares. To try whether the law, by which the magnetiſm alters, could be reduced at all diſtances, to any one certain power of thoſe diſtances, he ſought thoſe points in the needle and ſtone, which being us'd as the centers of the power, might have that property. But in that caſe he found the center of the ſtone muſt be carried quite out of its figure, to make the diſtances large enough for this purpoſe: Whence it ſeems to be evident, that the power of magnetiſm does not alter according to any particular power of the diſtances; but decreaſes much faſter in the greater diſtances than it does in the near ones.

This ſeems to be confirm'd by other experiments the Dr. made; the firſt experiment was this, he made a needle $\frac{3}{4}$ of an inch long, of very fine ſteel wire (a foot length weigh'd but a grain) which he lengthened by ſticking a light piece of ruſh to it; ſo that he could obſerve the direction of the needle in all the trials with a radius of two inches: Inſtead of a magnet he made uſe of a touch'd needle of ſteel wire, which he ſet on a perpendicular to the horizontal plane he made the obſervations on, by means of a frame, to tranſport it from one place to another; the north end of the needle being placed downwards and made a little ſharp, that it might mark the paper it was ſet upon in every poſition, by preſſing the top of the needle gently with the finger. The obſervations were made in this manner; after having taken notice of the natural direction of the ſmall compaſs-needle, he brought the perpendicular needle as near it as he could conveniently, ſetting it in ſuch a manner, that a line from the upright needle

to

to the center of the compaſs might be perpendicular to the compaſs-needle. Then obſerving the ſame caution (which was convenient to make the center of the compaſs ſerve ſufficiently well to be eſteemed its center of power) he placed the upright needle at ſeveral large diſtances, every time marking the place in the manner already deſcribed, and obſerving the variation of the compaſs: By this means he got a curve pretty regularly and fairly drawn by points on the paper; and by examining this curve, compared with the variations of the compaſs, correſponding to its reſpective points, he found, that the magnetical power decreaſed faſter at the greater than at the nearer diſtances: The Dr. obſerves, that at about two inches and $\frac{1}{4}$ diſtance, the force did not alter ſo faſt as the ſquares; and at 10 inches diſtance (where the variation was one degree only) it altered faſter than the cubes, the index of the power being about 3 and $\frac{1}{4}$. The needle of the compaſs was ſo ſhort, that to ſuppoſe its center of force to be either in the middle or extremity thereof, would not alter the index of the powers of the diſtances, $2\frac{1}{5}$ of an unite.

He made another experiment to the ſame purpoſe, with a compaſs needle made of a ſlight piece of ſtraw, with a little piece of ſteel-wire, faſtened to one end of it, which was always kept in the ſame poſition, being balanced between two perpendicular needles, one of which was moveable and the other fixed; the event was much the ſame as in the former experiment,

Endeavouring to find the true poles or centers of the magnetical force in touch'd needles, he made a needle of two inches long, of the fine ſteel-wire, which he touched with the ſouth point of a ſmall capt loadſtone, applying the point of the cap to the extremity of the needle, without drawing it along: The needle ſo touched, being laid gently on the ſurface of a ſtagnant water, floated; he then applied to it ſucceſſively the two ends of a touched needle, as near as he could, without letting the needles touch: The reſult was, that the floating needle reſted under the reſpective poles of the other needle, marked with the ſmall letters *s n s*, as repreſented in Fig. 14. Plate IV. ſo that by one touch with the loadſtone, which gave the needle a north pole at N. where it was touched, it acquired three other poles *s n s*, which may not, therefore, be improperly called its conſequential poles: Having diſcovered theſe conſequential poles, he made ſome other experiments to diſcover more of the nature of them, as repreſented in the Fig. The needles were all of them two inches long, made of the ſame fine ſteel wire; and the letter N or *n* and S or *s* denote north or ſouth, belonging to the points marked; the

great letters denoting the points the loadſtone was applied to, and the ſmall letters ſhewing the conſequential poles.

As to the Dr's experiment relating to the hyperbola made by the ſurface of water between two glaſs-planes; vide *Phil. Tranſ.* N° 336.

Dr. *Taylor* took ſeveral very thin pieces of fir-board, having hung them ſucceſſively in a convenient manner to a nice pair of ſcales, he tried what weight was neceſſary (over and above their own, after they had been well ſoaked in water) to ſeparate them at once from the ſurface of ſtagnating water: He found 50 grains to ſeparate a ſurface of an inch ſquare; and the weight in every trial being exactly proportional to the ſurface, he was apt to think the experiment well made: The diſtance of the under ſurface of the board from the ſurface of the ſtagnating water, at the time they ſeparated, he found to be $\frac{1}{100}$ of an inch; tho' he thinks it would be found greater, if it could be meaſured at a greater diſtance from the edge of the board, than he could do it, the water riſing a little before it came quite under the edge of the board.

A Method of determining the Places of the Planets, *by obſerving their near appulſes to the fixed* Stars; *by Dr.* Halley. Phil. Tranſ. N° 369. p. 209.

OF all the celeſtial obſervations hitherto made, none are ſo capable of perfect exactneſs, as the near appulſes of the moon and planets to the fixed ſtars; ſuch as lately we had of *Jupiter* to two ſmall ſtars in *Gemini* and of *Mars* to the forehead of *Scorpio*: For, tho' the places of the ſtars have not hitherto attained an ultimate preciſion; yet theſe ſorts of obſervations are ever good and the places of the planets are thereby aſcertained, in proportion to the correctneſs of the catalogues, that may hereafter be made: But the ordinary number of the ſtars, with which the planets may be thus compared, being ſmall, the opportunities of obſerving are conſequently rare: Whence appears the great uſe of a compleat catalogue of teleſcopic ſtars, at leaſt within the limits of the zodiac; *viz.* that thereby theſe opportunities may be more frequent: And wherever ſuch obſervations have formerly been made to theſe ſmall ſtars, we may be enabled to find them out; and by determining their places, to be alſo certain of the places of the planets; of which the Dr. gave a notable inſtance in finding the place of the great comet in 1680, in its firſt appearance, even before it had a tail viſible to the naked eye, of

which

which an account is given in *Phil. Transf.* N° 342. And since the Royal Observatory at *Greenwich* has been put under the Dr's care, he has endeavoured to put himself into a condition to supply the many and great vacancies to be met with in the present zodiac; and particularly, he has sought out and settled the places of two telescopic stars, to one of which *Jupiter* was observed by *Galilæo* to apply at the beginning of *March* 1610 N. S. and which is the very first observation of that kind that was made with the telescope, vide *Nuncius Syder.* p. 27, *edit. prin.* 1610. On the 28. of *Feb.* an hour after sun-set, a small fixed star was in conjunction with the fourth satellite (as it since appears to have been) being then eastwards of the planet. The next day *March* 1st at the same hour, the center of *Jupiter* was in the angle of an equilateral triangle with the fourth satellite and the star: And again *March* 2. *Jupiter* being retrograde, had past the conjunction of the star; and a line from the star, perpendicular to that of the satellites, fell on the first satellite then two minutes to the west of the planet, and in Lat. the star was more southerly than the satellite 8 mintues: The Dr. found out this star, by the direction of the place of *Jupiter* at that time; and by comparing it with others in the catalogue, having nearly the same declination, he settled its place in 13° 24′ and ½ of *Gemini* to the time of the *British* catalogue with 25′ S. Lat.

Another remarkable observation of *Saturn* is recorded in *Riccioli Astron. Reform.* p. 286, said to have been made at *Modena* by the Marquis *Malvazzo* on *July* 3, 1662, N. S. when the eastern *ansa* of *Saturn* touched a fixed star: By the then place of *Saturn*, the Dr. looked out for this star to which *Saturn* was then very near; and after the same method he settled its place in the beginning of the year 1690 (the *epocha* of the *Catal. Brit.*) in 29° 34′ of *Scorpio* with 2° and ½ a minute N. Lat. By this it will appear, how defective the observed place of *Saturn* is stated in *Riccioli*, there being an error of upwards of 7′ in the Lat. thereof.

An Observation of a Parhelium; *by the Same.* Phil. Transf. N° 369. p. 211.

OCTOBER 26. 1721, Dr. *Halley* being on the river, coming up to *London* about ½ an hour after 10 o'clock in the morning; the sun being then about 20 degrees high, he observed a circle about the sun, which is by no means unusual, when the air in chilly weather, such as it was then, is replete

with ſnowy particles; which circle was of the ſize it always appears in, about 23 degrees from the ſun and faintly tinged with the colours of the *iris*. When this circle happens, the Dr. always looks out to obſerve, whether any other of the phænomena, that ſometimes attend it, do at that time appear; ſuch as parhelia and other coloured circles, concentric with the ſun; and ſometimes, as he once obſerved it, excentric; as alſo a white circle round the zenith, of equal altitude with the ſun: But at this time the air being thickened with a hazy vapour and the ſmoke of *London*, he could only obſerve to the eaſtward a luminous white patch, which, for about 20 minutes ſhone very conſpicuouſly thro' the thick air, about two degrees in diameter, as near as he could eſtimate, and about the ſame altitude with the ſun; and from it towards the ſun, there ſeemed to iſſue a long white tail much narrower than the mock-ſun; but which he took to be a ſegment of the white circle, which he once obſerved entire at *London*: Had the air been clear, the Dr. doubts not but much more of the phænomena of the parhelia might have been obſerved at that time: But how to explain theſe appearances and account for the magnitude of theſe circles is what ſeems ſtill to be wanting.

An Account of two Mock-ſuns *and an arch of a* Rain-bow *inverted; together with an* Halo *and its brighteſt arch, obſerved at* Lyndon *in the County of* Rutland; *by Mr.* Whiſton. Phil. Tranſ. N° 360. p. 212.

OCTOBER 22. 1721, about 10 o'clock in the morning, Mr. *Whiſton*, being at *Lyndon* in the county of *Rutland*, after an *aurora borealis* the night before (wind W. S. W.) he obſerved an attempt towards two mock-ſuns, as he had done at ſome other times: About $\frac{1}{2}$ or $\frac{3}{4}$ of an hour after, he went to view the heavens and then found the appearance compleat, when two plain parhelia or mock-ſuns were to be ſeen tolerably bright and diſtinct; and that in the uſual places, *viz.* in the two interſections of a ſtrong and large portion of an halo (as repreſented in Fig. 15. Plate IV.) with an imaginary circle, parallel to the horizon, paſſing thro' the true ſun: This circle he calls imaginary, becauſe it was not itſelf viſible, as it ſometimes has been at ſuch appearances: Each parhelion had a tail of a white colour, and in direct oppoſition to the true ſun; that towards the eaſt was 20 or 25 degrees long; that towards the weſt about 10 or 12 degrees; but both were narroweſt at the remote extremities: The mock-ſuns were evidently red

towards

towards the ſun; but pale or whitiſh at the oppoſite ſides, as was alſo the halo: Upon caſting his eyes upward, he obſerved an arch of a curious inverted rain-bow, about the middle of the diſtance between the top of the halo and the vertex, that is, when allowance is made for the uſual unequality, that appears between the ſame number of degrees, nearer to and remoter from the vertex: This arch was as diſtinct in its colours, as the common rain-bow; and with the like allowance as before, of the ſame breadth: The red colour was on the convex, and the blue on the concave of the arch, which ſeemed to be about 90 degrees long; its center in or near the vertex: On the top of the halo was a kind of inverted bright arch, tho' its bend was not diſtinct: The lower part of the halo was among the vapours of the horizon and inviſible. The angles, as more exactly meaſured next day, near noon (when the ſame appearance returned again, but more faintly) were as follows; the ſun's altitude was 22° and $\frac{1}{3}$; the perpendicular ſemidiameter of the halo 23° and $\frac{1}{3}$; the diſtance of the rainbow from the top of the halo 23° and $\frac{1}{3}$; the ſemidiameter of the arch of the rainbow, ſuppoſing the vertex its center, 21°. The phænomenon laſted each day for 1 hour and $\frac{1}{2}$ or two hours. What was moſt remarkable on the 23. was that the wind, which on the 22. was almoſt inſenſible, was now become ſenſible and changed to N. N. E. ſo that the halo was ſenſibly become oval; its ſhorter axis parallel to the horizon; and the two mock-ſuns, which were then but juſt viſible, eſpecially that on the eaſt, were not in the halo, but a degree or two without it, which Mr. *Whiſton* aſcribes to the unuſual ſhortneſs of the horizontal diameter; and this poſition of the mock-ſuns does not appear to have been taken notice of by any, tho' it was now very ſenſible.

October 26. About 9 o'clock in the morning, as Mr. *Whiſton* was coming in the *Northampton* coach towards *London*, the halo returned larger and clearer than before; and the two mock-ſuns juſt attempted an appearance therein, as on the 22. but the air becoming thicker and thicker towards rain, he ſaw them no more.

Auguſt 30. before, he obſerved at *Lyndon* in *Rutlandſhire* a remarkable halo, whoſe upper part had its inverted arch reddiſh within and pale without, but brighter and more vivid than ever he obſerved it in his life; and at the ſame place *Sept.* 11. in the evening there was the brighteſt and moſt remarkable *aurora borealis*, with its unaccountable motions and ſhiftings,

ſhiftings, that ever he obſerved; excepting that original one *March* 6. 1715-16; this *aurora* was ſeen in *Northamptonſhire*, at the *Bath* and elſewhere; the vertex of the columns, which ſhot upwards, was not his vertex, but evidently 15 or 20 degrees diſtant towards the ſouth; and in *Rutland* the wind was north, at the *Bath* weſt, and in *Northamptonſhire* ſouth, all at the ſame time.

As to thoſe northern lights, Dr. *Halley* ſoon after their firſt appearance, communicated his thoughts to the public in a former *Tranſaction*, and Mr. *Whiſton* in a ſmall pamphlet, publiſhed about the ſame time; and as to the halo's, mock-ſuns, inverted arches of rain-bows and other phænomena of the like nature; M. *Huygens* has very accurately explained them in his *poſthumous works* from p. 293. to p. 366. and Sir *Iſaac Newton* has touched upon them in his *Optics* 1. *edit.* *p.* 134. As to the unuſual frequency of the northern lights of late Mr. *Whiſton* was far from ſatisfying himſelf.

Obſervations on the Generation of Plants; *by Dr.* Blair. Phil. Tranſ. N° 369. p. 216.

WHAT Dr. *Blair* had advanced in his *Botanic eſſays* is fully confirmed by experiments made by ſome curious gardeners, particularly, Mr. *Philip Miller*, who writ him an account 1. That *Nov.* 11. 1721, in purſuance of the Dr's advice, he ſeparated the male plants of the ſpinage from the female: the conſequence was, that the ſeeds ſwelled to the uſual bigneſs; but that they did not grow when he ſow'd them: He examined the ſeed, and found they wanted the *punctum vitæ*, which perhaps might have been the caſe with M. *Geoffroy*; but if not, the female embryo's might have been impregnated another way, as he tried with 12 tulips, which he ſet by themſelves about ſix or ſeven yards from any other; and as ſoon as they blew, he took out the *ſtamina* ſo very carefully, that he ſcattered none of the duſt; and about two days after, he obſerved bees working on tulips in a bed, where he did not take out the *ſtamina*; and when they came out, their bodies and legs were loaded with the duſt; he obſerved the bees fly into the tulips, where he had taken out the *ſtamina*; and when they came out, he found they had left enough behind them to impregnate theſe flowers; for, they bore good ripe ſeed; by which he is perſuaded, that the *farina* may be carried

ried from place to place by insects; and when they happen upon a flower, whose *uterus* is capable of being impregnated by such a dust, it may be thus effected.

The Dr. is of opinion, that this will not suit with Mr. *Morland*'s scheme: For, tho' we may suppose the *stamina* of every flower to be loaded with a due proportion of the *farina*; yet this accidental conveyance of it to a neighbouring flower may be rather less than greater than is necessary: So that if wanting then those embryo's, which had not received the determined particle into their bosom, must be defective in bulk or barren in growing; but here all were equally filled.

2. Mr. *Miller* in another letter, dated *October* 19. 1721, informs the Dr. that he bought a parcel of *Savoy* seeds, which he sowed, and planted out the plants; but was surprised at the production. For, he had half of them red cabbages; some savoys with red ribs; and some neither one sort nor the other, but a mixture of all sorts together in one plant. The same thing happened to the gardener of whom he had the seed, but he did not know how; for, he was sure he took special care in saving of the seed; he sowed them under a south-west hedge, and planted them in the following manner; first a dozen of white cabbages, then a dozen of savoys and then a dozen of red: Upon which Mr. *Miller* immediately thought, that this was owing to the effluvia impregnating the *uterus* of each other; and it is very common for our gardeners to plant white and red cabbages together for seed; and they are as often disappointed by having a degeneracy of both kinds, which they attribute to the soil; and they send to *Holland* for a fresh supply of seed and affirm that our soil will not continue that sort good.

This experiment is a most convincing argument for the effluvia; for, did each grain of the *farina* enter the *pistillum* to its proper *uterus*, this mongrel kind would never be produced: For, if the individual plant be in each grain of the male *farina*, how can it be so far dismembered, as that one part shall go to the making up the ribs of red cabbage and another to compose the rest of a savoy plant.

Analogous to this is what the Dr. observed of a spaniel bitch, of a good kind; when she became proud, care was taken to let her have good dogs; the litter she produced consisted of puppies, some pieball'd, like one of the dogs that had lin'd with her, of the same shape, colour and spots; others

others like another; and a third partaking of both, with ſpots of the bitch interſpers'd.

This is a farther confirmation of what the Dr. has advanced *Eſſay* 4. where p. 310. he aſſerts, that ſeveral *fœtus*'s partake equally of male and female: But here two males concur with one female in the compoſition of a fourth body, made up of all the three; and one ſeed produces a cabbage, conſiſting of three different ſpecies, which could never happen, did theſe organiz'd *animalcula* or ſmall grains of the *farina* become a *fœtus* or contain the *folia ſeminalia* of a plant: This is ſufficient to anſwer what the ingenious Mr. *Bradley* has ſo ſtrenouſly contended for, in his *Works of Nature* p. 9. ſeqq.

It is worth while to conſider, how far this obſervation may lead us into the infinite variegations and ſtripes, not only in annual flowers, ſuch as poppies, *conſolida regalis* and bottles, but alſo in perennial roots; ſuch as *auricula*'s, cowſlips, *&c.* of a lower ſize, which is hinted by Mr. *Bradley*, having receiv'd that notion from the ingenious M. *Du Bois*; and in plants of a larger ſize, not of a bulbous but carnous root, ſuch as columbines; where there is a vaſt variety: And in this plant it is moſt eſpecially to be obſerv'd, that tho' the native one, from which all the other ſeem only to be variations and not determinate ſpecies, be of a blue colour, conſiſting of 10 alternate *petala*, viz. five corniculate and five plain: Yet into how many other kinds of flowers is it ſubdivided? Such as pale yellow, with bluiſh red, purple, dark double ſtripes, blue, blackiſh red, *&c.* Some with corniculate *petala*, and ſome only with plain; and how in ſingle flowers it imitates all the colours obſervable in a pidgeon.

It is worthy conſideration, whether the *farina* may do this; ſince it does not appear, that there has been much art us'd in making theſe flowers break, as tulips, or to cultivate a ſet of breeders; but that a richer ſoil may produce a double flower; and a ſuitable loam may produce the variety of colours: The *farina* from ſeveral flowers may occaſion the ſtripes; and the *ſtamina* ariſing from the plain *petala*, rather than the *cornicula*, pouring out the *farina*, may cauſe the flowers with the plain *petala*. So that were this to be extended to a great many other plants and were there proper obſervations made upon them, conſiderable improvements might be made in this doctrine of the ſexes of plants: After the flowers,

we

we might conſider next the variegation of the ſeed of ſome plants, particularly the *phaſeoli*, whoſe various ſpots and colours, and even their bigneſs too, may very much depend upon the effluvia from the *farina*, when ſeveral kinds are ſown together: For, if we conſider three plain colours, as a white, red and dark blue, we may obſerve how many deſcendants, and what a variety of ſpots may proceed therefrom The lupines may likewiſe in ſome meaſure be brought in here; and perhaps, the *Medica cochleata falcata lunata* may be multiplied in its variegations after the ſame manner.

Mr. *Miller*, being perſuaded to it by an ingenious gardiner, pull'd off all the male flowers of ſome melon plants, as ſoon as they appeared; but inſtead of finding, as his friend inform'd him, that theſe flowers exhauſted the nouriſhment from the fruit, he found, that without theſe flowers none of the melons would grow; ſo that he was depriv'd of the fruit he expected.

As this experiment is a plain indication of the neceſſity of the *farina*; ſo it confirms the uſe the Dr. has aſſigned to the leaves, *viz.* that the nutritive particles by entering their capillaries and returning again, may be more attenuated: So here the *petala* of the male flowers may ſerve for the ſame purpoſe; for, by the largeneſs of the *tubuli* in theſe *pomiferæ ſcandentes*, a groſs viſcid ſap is receiv'd, which even the leaves themſelves are not ſufficient to attenuate, ſo as to be fit for compoſing the more ſubtile part of the fruit; till by repeated circulation thro' the *petala* of the male flowers it may be render'd fit for ſuch a purpoſe: It is true, that the female flowers upon the top of the *rudimenta fructûs* may in ſome meaſure ſerve for this purpoſe; but as the male flowers are, generally ſpeaking, more numerous than the female; ſo their being remov'd muſt deprive the embryo's of a very great aſſiſtance towards being perfected: The Dr. farther adds, that the orifices of the pedicles, when the flowers are pull'd off, muſt loſe ſo much of the ſap, that the whole plant muſt be thereby impoveriſh'd in ſuch a manner, as not to be able to bring forth the deſign'd fruit; and all this, beſides the want of the conſiderable ſupply of the *farina fœcundans*.

An Obſervation of an extraordinary height of the Barometer; *by Mr.* George Graham. Phil. Tranſ. N° 369. p. 222.

ON the 21. of *December* 1721, Mr. *Graham* obſerving the barometer much higher than uſual; between 7 and 8 o'clock that evening he fill'd a tube with very clean quickſilver, and found the height to exceed ſomewhat 30, 7 ½ inches: By 8 o'clock next morning, a wheel-barometer, which hung in the ſame room, had riſen one tenth of an inch higher than it was the night before, when the experiment was made; at 10 o'clock, one fifth of an inch more: At which time it was at the higheſt, being a little above 30, 8 ½ inches; about 12 at noon it was ſenſibly lower and continued falling all the reſt of the day.

When the lower end of the tube was firſt immers'd in the ciſtern, the quickſilver adher'd for ſome time to the crown of the glaſs; but upon ſhaking, it fell to the above-mentioned height.

A Caution to be us'd in examining the ſpecifick Gravity of Solids, when weigh'd in Water; by Dr. Jurin. Phil. Tranſ. N° 369. p. 223.

THAT the experiments for finding the ſpecific gravity of ſolid bodies may be made with exactneſs ſo as to draw any inferences from them in natural philoſophy, Dr. *Jurin* gives the following caution, *viz.* that when a dry porous body is to be weigh'd in water, in order to diſcover its ſpecifick gravity; it is neceſſary, by ſome means or other, to extricate the air out of all the ſmall pores and vacuities within it, that the water may have free liberty to pervade them; unleſs this care be taken, it muſt needs happen, that the air, which poſſeſſes thoſe ſmall cavities and keeps the water out, will render the ſolid of leſs weight in the water; and conſequently, of leſs apparent ſpecifick gravity than it really is.

The beſt way of avoiding this inconveniency is, to ſet the veſſel of water, in which the ſolid body is immers'd, under the receiver of an air-pump, and to exhauſt the air out of the body by that means; which will be more eaſy and exactly done, if the water be firſt heated over the fire: And where the conveniency of an air-pump cannot be had, the ſame thing may be done almoſt as well, by letting the ſolid body continue ſome time in boiling water over the fire.

But

But no ſolid body muſt ever be put into hot water, that will in any meaſure diſſolve or give a tincture to the water.

One inſtance of the neglect of this caution may be obſerv'd in the accounts we have of the ſpecifick gravity of the ſtones extracted from human bladders, which have been commonly found to be but about half, and ſome of them no more than $\frac{1}{4}$ part heavier than an equal bulk of water. From this it has been too haſtily concluded, that theſe ſtones are very improperly call'd by that name, as not at all approaching to the ſpecifick gravity of even the lighteſt real ſtones, we have any account of.

Whereas it is much more reaſonable to ſuppoſe, that thoſe ſtones, which have been found to be ſo light, were ſuch as had been for a conſiderable time extracted out of the bladder; and conſequently, had loſt much of their weight by the evaporation of the urine, with which they had at firſt been ſaturated, and that they had afterwards been tried without the caution abovementioned. The Dr. would, therefore, recommend it to thoſe, who ſhall examine the ſpecific gravity of the human *calculus*, that they would either try the experiment upon ſtones freſh extracted out of the bladder, or elſe uſe the aboveſaid method, to extricate the air out of their cavities; if they do this, the Dr. is ſure they will meet with ſome *calculi* (as he himſelf has done) exceeding the weight of ſome ſorts of burnt earthen ware and alabaſter; and approaching very near to that of brick and the ſofter ſort of paving ſtone: But it is not to be expected, that they ſhould entirely equal the ſpecific gravity of ſtone, found in the earth; becauſe the mixture of ſome portion of the animal oil and volatile ſalt, with the ſtony ſubſtance of the human *calculi*, muſt needs leſſen the ſpecific gravity of the whole concrete.

Dr. *Jurin* was very much ſurpris'd at the following obſervation, he accidentally diſcovered, to wit, that the ſubſtance of all wood (as oak, fir, *&c.*) is ſpecifically heavier than water: To prevent being miſunderſtood, the Dr. obſerves, that in wood and other vegetables, there are two ſorts of veſſels; one of which convey the ſap; and the other contain only air, for which reaſon they are call'd air-veſſels: When wood floats or ſwims in water, this effect is not owing to the lightneſs of the ſubſtance of the wood, but only to its being buoy'd up by the air contain'd in the ſaid veſſels: For, when the air is exhauſted out of thoſe veſſels, and inſtead thereof the water has inſinuated itſelf into them, the wood will ſink to the bottom; as is evident in ſmall chips or ſhavings of wood, by means of the air-pump or an infuſion in boiling or even in cold water for a ſufficient time; and the ſame

is found to be the caſe in the roots, ſtalks, leaves and ſeeds of as many other vegetables as the Dr. had hitherto tried, cork only excepted; in which laſt he had no reaſon to expect it, conſidering the particular ſtructure of its ſubſtance, as deſcribed by the learned Dr. *Hook* in his *Micrographia*.

An Oſſification of the Crural Artery; *by Mr.* Edward Naiſh. Phil. Tranſ. N° 369. p. 226.

MR. *Conſett* of *Cleveland* in *Yorkſhire*, 67 years of age, who had all his life before enjoyed a perfect good ſtate of health, ſent for Mr. *Naiſh* on account of a mortification which began about a month before upon one of his toes; and by gradual advances in that time had reached half way up his leg; and this without any manifeſt cauſe: Mr. *Naiſh* found him in this condition, *viz.* with a perfect mortification or *ſphacelus* of his foot and half his leg: The patient ſaw himſelf dying daily by piecemeal, but heart-whole, as he expreſſed it, and with a pretty good pulſe. Mr. *Naiſh* propoſed amputation as the only remedy, which the patient readily conſented to.

The leg being cut off at the uſual place (which was four inches above the mortification) about two or three ounces of blood iſſued out from the muſcular part; but upon ſlackening the turneket, in order to look for, and tie up the artery, not one drop of blood flowed out, to Mr. *Naiſh*'s great ſurpriſe; and upon feeling the extremity of the artery, he found it hard and callous; however, he ſecured it by a ligature, as uſual, and dreſs'd the ſtump. The patient (who had borne the operation with the greateſt reſolution) being put to bed, Mr. *Naiſh* was deſirous to examine the leg; and having diſſected the artery, with its two conſiderable branches, as far as the *tarſus*, he found them for the moſt part oſſified; that is to ſay, the trunk, where it was amputated, was oſſified about $\frac{2}{3}$ of its circumference: About $\frac{1}{4}$ of an inch lower, the whole was bony, leaving ſo ſmall an orifice, that it could only admit of a hog's briſtle: A little lower, it was bony on one ſide; and membranous on the other; and then again entirely bony; here and there, for the breadth of a barley-corn, there was no bone at all. Mr. *Naiſh* opened about two inches of the internal branch immediately above the *malleolus*, it appearing blacker than the reſt; after it had been waſhed he found therein about two or three drops of coagulated blood, and now that it was expanded and dried, it was one entire *lamina* of bone, as thick as the ſhell of a pidgeon's egg and of an uneven ſurface: He diſſected three ramifications of this internal branch into the foot; only

only one of them had a very ſmall bone in it, about ½ an inch from the trunk: The other large branch, running on the ligament, that ties the ſociles together, was not ſo much oſſified as that already deſcribed.

This oſſification (the compleateſt Mr. *Naiſh* ever heard of) was, doubtleſs, the cauſe of the mortification, and of the death of the patient, which followed four days after the amputation.

This bony *lamina* or ſhell was on the inſide of the tunicles or coats of the artery. Mr. *Naiſh* doubts not but that theſe caſes are more common than is imagined: For, when we ſee mortifications ſeize the extreme parts of aged people, we commonly attribute them to a decay of nature, or an extinction of the vital warmth, tho' he takes this to be oftener the cauſe; and he is the more apt to think ſo, from two or three parallel caſes he had been acquainted with in his own practice.

An Account of a Rainbow *obſerved on the Ground; by Dr.* Langwith. Phil. Tranſ. N° 369. p. 229.

SEPT. 7. 1721, about 9 o'clock in the morning Dr. *Langwith* riding with ſome others over *Port-mead* near *Oxford*, and the morning miſty and graſs very wet with the dew; they had not been long out, before the air cleared up and the ſun began to ſhine very bright: Soon after, they obſerved a rainbow on the ground, whoſe colours were near as lively as thoſe of the common *iris*, extending for ſome hundreds of yards; the colours were ſo ſtrong, that it might have been obſerved much farther, had it not been terminated by the bank and hedge of the field: It continually changed its place as they moved along, which commonly happens in other rainbows; the more remarkable particulars were, as follows.

1. That its figure was not round, but oblong; being as Dr. *Langwith* took it, a portion of an hyperbola. 2. That its convex part was turned towards the eye, and the vertex at a ſmall diſtance before them. 3. That the colours took up leſs ſpace, and were much more lively in the nearer than remoter parts thereof.

Theſe phænomena may be eaſily accounted for, by comparing this *iris* D C E Fig 16. Plate IV. with the common *iris k i* E *e*, formed by drops falling in the air at a ſmall diſtance from the eye of the ſpectator H, and touching the ground with the lower part of its arch in E, the vertical point of the *iris* D C E; produce the cone H *k i* E *e*; its interſection with the plane of the horizon will give the figure of the *iris* D C E. Hence it follows.

1. That

1. That as the angle *e* H G happens to be greater, equal to, or leſs than 90 degrees, the figure will be an hyperbola, parabola or ellipſis. 2. That as the ſun was about 30 degrees high, when they view'd the phænomena, the *iris* was an hyperbola. 3. That the arches of the ſame *iris*, conſiſting of colours of different refrangibility, may alſo in ſome caſes be different ſections of the cone. 4. That ſince the angle *e* H F is always given; from the height of the point of view H G and the ſun's altitude S L A, the dimenſions of theſe *iris*'s are eaſily determined.

An Account of the Pores of the Leaves of Box; *as alſo of the* Down *of* Peaches *and* Quinces; *by M.* Leewenhoeck. Phil. Tranſ. N° 369. p. 231. *Tranſlated from the* Latin.

M. *Leewenhoeck* pulled off a leaf from that ſpecies of box, called *palma ceres*, and tearing it to pieces he viewed it with his microſcope; after which, he could very diſtinctly obſerve theſe parts, thro' which perſpiration is performed; beſides a great many very ſmall pores, that tranſmitted the light; and which appeared to greater advantage, and more numerous, as the parts of the leaf were ſomewhat drier.

In the ſmall leaves of a box, that were partly green and partly withered, he perceived the pores ſtill more diſtinctly than he had ever done in any others; and the more diſtinctly to repreſent their great number, he laid a box leaf on a braſs ruler divided into ſeveral parts; and he found its length equal to $\frac{8}{10}$ parts of an inch (ſuppoſing an inch divided into 10 parts) and its breadth to $\frac{1}{10}$ parts.

Now ſuppoſing the figure of ſuch a leaf to be oval, and adding its length and breadth together, the ſum will be 13, whoſe half is 6 and $\frac{1}{2}$: Again ſuppoſing the ſame leaf, after adding its length and breadth together, to be round like a circle, its diameter is equal to 6 tenth parts and $\frac{1}{2}$ of an inch.

Afterwards M. *Leewenhoeck* laid a hog's briſtle cloſe by the ſaid leaf; and viewing it with his microſcope, he judged that 12 pores of the leaf, if contiguous to each other, would be equal to the diameter of the briſtle; and that 60 briſtles would make an inch; whence it follows, that each tenth part of an inch is equal in length to ſix diameters of a briſtle; but the ſemidiameter of a ſmall leaf of box to 19 diameters and a half of a briſtle; which 19 diameters and $\frac{1}{2}$ if multiplied by 12, the number of pores, the product will be 234, which, from what has been ſaid, is the length of a ſemicircle of box leaf.

In

In order to find the ſuperficial contents of ſuch a circle; we muſt in the firſt place ſuppoſe with geometricians, that as 14 to 11, ſo is the ſquare of the diameter of any circle to the contents thereof: Hence it follows, that one ſuperficies of a box leaf is furniſhed with 172090 pores; and ſince the other is furniſhed with no fewer, the whole number of pores for perſpiration will be 344180.

M. *Leewenhoeck* obſerved with his naked eye the down on peaches, commonly called mountain-peaches; he placed before his microſcope ſome pieces, he had cut off from the rind; and he judged the down particles equal in number to the pores in the rind; and as he had formerly hinted, that the pores in fruits opened between two ſmall lips, as it were, and were not quite round, but ſomewhat oblong; ſo in like manner the down, iſſuing out of the ſaid pores, is not quite round, but ſomewhat flat; yet he obſerved a great many in the middle ſomewhat corrugated.

In order to ſhew the vaſt number of pores deſigned for perſpiration, as alſo the exhaling humour, which emitted into the air, is inſpiſſated into oblong particles, but ſome longer than others, M. *Leewenhoeck* cauſed delineate a ſmall portion of a peach, as repreſented by A B C D E F G Fig. 17. Plate IV. where A B F G is a little bit of the fruit and rind; B C D E F the down adhering to the peach.

The bulk of the ſaid little piece of peach may be eſtimated thus; M. *Leewenhoeck* took three hairs of his face, and viewing them with his microſcope; after dividing the length of the figure from G to A into 16 parts, the ſaid length of the figure was found not to exceed eight diameters of a hair: Which if true, what an incredible number of down particles muſt neceſſarily cover a peach?

Afterwards M. *Leewenhoeck*, viewing with his microſcope ſome particles of a ripe quince, cauſed delineate the down thereon, which is no leſs copious than on a peach, as repreſented by H I K L M N O Fig. 18; where H I N O is a ſmall portion of the pulp and rind; I K L M N the down exſudating therefrom, which, tho' longer in quinces than in peaches, does not ſtand erect in the former as in the latter, but is matted together.

Remarks

Remarks on ſome attempts towards a perpetual Motion; *by Dr.* Deſaguliers. Phil. Tranſ. N° 369. p. 234.

THE wheel at *Heſſe-Caſſel*, made by M. *Orfireus* and by him called a perpetual motion, has been ſo much talked of, on account of its ſurpriſing phænomena, that a great many have taken it to be actually a ſelf-moving engine, and accordingly attempted to imitate it as ſuch; Dr. *Deſaguliers* ſhews, that the principle, moſt of them go upon, is falſe and can by no means produce a perpetual motion.

They take it for granted, that if a weight, deſcending in a wheel, at a determinate diſtance from the center, does in its aſcent approach nearer to it; ſuch a weight in its deſcent will always preponderate and cauſe a weight equal thereto to riſe, provided it comes nearer the center in its riſe; and accordingly as itſelf riſes, will be overbalanced by another weight equal thereto; and, therefore, they endeavour by various contrivances to produce that effect, as if the conſequence thereof would be a perpetual motion.

The Dr. ſhews, that they miſtake one particular caſe of a general *Theorem*, or rather a *Corollary* of it for the *Theorem* itſelf, which is as follows.

Theor. If one weight in its deſcent, does, by means of any contrivance, cauſe another weight to aſcend with a leſs *momentum* or quantity of motion than itſelf, it will preponderate and raiſe the other weight.

Cor. 1. Therefore, if the weights be equal, the deſcending weight muſt have more velocity than the aſcending, becauſe the *momentum* is made up of the weight, multiplied into the quantity of matter.

Cor. 2. Therefore, if a leaver or balance, have equal weights faſtened or ſuſpended at its ends, and the *brachia* be ever ſo little unequal, that weight will preponderate, which is fartheſt from the center.

Scholium. This ſecond *Corol.* cauſes the miſtake; becauſe thoſe, who think the velocity of the weight is the line it deſcribes, expect that that weight ſhall be overpoiſed, which deſcribes the ſhorteſt line; and therefore contrive machines to cauſe the aſcending weight to deſcribe a ſhorter line than the deſcending weight does: As for example, in the circle A D B *a* Fig. 19. Plate IV. the weights A and B, being ſuppoſed equal, they imagine, that if (by any contrivance whatever) whilſt the weight A deſcribes the

the arch A *a*, the weight B is carried in any arch, as B *b*; ſo as to approach nearer the center in its riſing, than if it went up the arch B D; the ſaid weight ſhall be overpoiſed; and conſequently, by a number of ſuch weights, a perpetual motion will be produced.

This is attempted by ſeveral contrivances, which all depend on this falſe principle; the Dr. only mentions one, repreſented by Fig. 20. where a wheel with two parallel circumferences, has the ſpace between them divided into cells; which being carved, will (when the wheel goes round) cauſe weights placed looſe in the ſaid cells, to deſcend on the ſide A A A, at the outer circumference of the wheel; and on the ſide D to aſcend in the line B *bbb*, which approaches nearer the center and touches the inner circumference of the wheel: In a machine of this kind, the weights, it is true, will move in ſuch a manner, If the wheel be turned round, but will never be the cauſe of the wheel's going round. Such a machine is mentioned by the Marquis of *Worceſter* in his *Century* of inventions N° 56 in the following words.

'To provide and make that all the weights of the deſcending 'ſide of a wheel, ſhall be perpetually farther from the center 'than thoſe of the mounting ſide; and yet equal in number and 'heft to the one ſide as well as the other; a moſt incredible 'thing if not ſeen; but tried before the late king (of bleſſed 'memory) in the *Tower* by my directions, two extraordinary 'Ambaſſadors accompanying his Majeſty, and the Duke of '*Richmond* and Duke of *Hamilton*, with moſt of the court attending him. The wheel was 14 foot over, and had 40 weights 'of 50 pounds a piece. Sir *William Balſore*, then lieutenant 'of the *Tower*, can juſtify it with ſeveral others. They all 'ſaw, that no ſooner theſe great weights paſſed the diameter line 'of the lower ſide, but they hung a foot farther from the center; 'nor no ſooner paſſed the diameter line of the upper ſide, but 'they hung a foot nearer. Be pleaſed to judge the conſequence.'

Now the conſequence of this and ſuch like machines, is nothing leſs than a perpetual motion; and the fallacy is this. The velocity of any weight is not the line, which it deſcribes in general, but the height it riſes up to or falls from, with reſpect to its diſtance from the center of the earth: So that when the weight in Fig. 19. deſcribes the arch A *a*, its velocity is the line A C, which ſhews the perpendicular deſcent (or meaſures how much it is come nearer the center of the earth) and likewiſe the line B C denotes the velocity of the line B, or the height that it riſes to, when it aſcends in any of the arches B *b*, inſtead of the arch B D:

So that in this caſe, whether the weight B in its aſcent be brought nearer the center or not, it loſes no velocity, which it ſhould do, in order to be raiſed up by the weight A; nay, the weight in riſing nearer the center of a wheel may not only not loſe of its velocity, but be made to gain velocity, in proportion to that of its counterpoiſing weights, that deſcend in the circumference of the oppoſite ſide of the wheel; for, if we conſider two radii of the wheel, as repreſented by Fig. 21. one of which is horizontal and the other (faſtened to and moving with it) inclined under the horizon in an angle of 60 degrees, and by the deſcent of the end B of the radius BC, the radius CD by its motion cauſes the weight at D to riſe up the line *p* P, which is in a plane that ſtops the ſaid weight from riſing in the curve DA, that weight will gain velocity, and in the beginning of its riſe, it will have twice the velocity of the weight at B; and conſequently, inſtead of being raiſed, will overpoiſe, if it be equal to the laſt mentioned weight; and this velocity will be ſo much the greater, in proportion as the angle ACD is greater, or as the plane P*p* (along which the weight D muſt riſe) is nearer to the center: It is true if the weight at B Fig. 19. could by any means be lifted up to *β*, and move in the arch *βb*, the end would be anſwered; becauſe then the velocity would be diminiſhed and become *β*C.

Experiment. Take the leaver BCD (as in Fig. 21.) whoſe *brachia* are equal in length, inclined in an angle of 120 degrees at C and moveable about that point as its center: In this caſe a weight of two pounds, ſuſpended at the end B of the horizontal part of the leaver, will keep *in æquilibrio*, a weight of four pounds ſuſpended at the end D: But if a weight of 1 pound be laid upon the end D of the leaver; ſo that in the motion of D along the arch *p*A, this weight is made to riſe up againſt the plane P*p* (which equally divides the line AC equal to CB) the ſaid weight will keep in *æquilibrio* two pounds at B, as having twice its velocity, when the leaver begins to move. This will be evident, if you let the weight 4 hang at D, while the weight 1 lies above it: For, if you then move the leaver, the weight 1 will riſe 4 times as faſt as the weight 4.

A Method for Rowing Men of War in a Calm; *by M.* Du Quet, *together with an Experiment and Remarks thereon; by M.* De Chazelles. Phil. Tranſ. N° 369. p. 239.

TO perfect the art of navigation, two things ſeem principally wanting; *viz.* an eaſy method for finding the longitude at ſea; and a way to give a veſſel its courſe, when there is no wind ſtirring.

M. *Du*

M. *Du Quet* flatters himſelf to have found the laſt and hopes to make it appear, by reaſon and experiment, that a man of war may make a league an hour in a calm, by means of revolving oars, which are eaſily applied to the ſides of a ſhip, without occaſioning any incumbrance; as appears by the following account, after delivering his notion of the motion of bodies in fluids.

A body ſwims upon water, when it weighs leſs than the volume of water, whoſe place it takes up; and it ſinks more or leſs in the water, only in proportion as its volume is more or leſs increaſed.

A body lying in ſtill water is in *æquilibrio*, as it were; the leaſt effort gives it motion and makes it loſe that *æquilibrium*: If the effort be continued, tho' ever ſo little, the motion it communicates will be very ſenſible: How great ſoever the weight of the body be, when once it is in motion, it will always continue ſo, if nothing hinder it.

Upon theſe principles M. *Du Quet* conſiders the motions a veſſel receives by means of oars, and the application of hands that ſet it a-going: The *impetus* of the hand, applied at one end of the oar, and the reſiſtance the water makes againſt the other end, are both impreſſed upon the point, where the oar reſts upon the veſſel: This point is like the *fulcrum* of the common leaver, which always bears the ſum of the weights at both ends, beſides the weight of the leaver itſelf: So that the greater the effort is at one end of the oar, and the reſiſtance at the other end; ſo much the greater is the impreſſion, which the point or *fulcrum* receives, in order to its being put into motion: A galley with two oars only, would go as faſt as it does with the uſual number; provided the ſame number of hands were applied with equal vigour to the two oars; and the oars were ſtrong and broad enough to make the neceſſary reſiſtance; becauſe then the *fulcrum* of the two oars would receive as much impreſſion, as all the *fulcra* of the common oars taken together.

This conſideration put M. *Du Quet* at firſt upon contriving a way, how to apply a great number of hands to the common inclined oars; but after ſeveral trials he threw them aſide and made uſe of perpendicular oars; becauſe the firſt do only ſkim the water, and (when the ſea is rough and the waves run high) they oftentimes do not take water; and ſo become uſeleſs: For, in this caſe, the rowers are tript up for want of meeting a reſiſtance.

This inconveniency is avoided by the revolving oars; becauſe they take the water perpendicularly and enter far enough not to miſs it; and if the water ſhould happen to evade the ſtroke, the rowers would not be ſo incommoded; becauſe they would be ſupported at every vibration, which is only of three foot: Beſides, in the uſe of inclined oars, more than half the time is loſt, in raiſing and recovering the oar, before they give the ſtroke; which makes the veſſel move by jerks and fits; ſo that the people aboard feel, as it were, every ſtroke of the oars when they play; whereas, the revolving oars always move equally, and ſucceed each other without loſs of time; which makes the veſſel move uniformly, without affecting thoſe who are aboard.

It is likewiſe to be obſerved, that a galley built on purpoſe for the uſe of inclined oars, would not be ſo proper as another veſſel built for perpendicular oars; becauſe the galley has a conſiderable length and but little height above the water.

M. *Du Quet*, having propoſed this invention to the court of *France*, was ſent to *Havre de Grace*, to make a trial, which had the approbation of the intendant, who made his report, that the officers at firſt objected to the invention; but as for his own part, the more particularly he conſidered it, the more he was convinced of its uſefullneſs. M. *Du Quet* was afterwards ſent to *Marſeilles*, where he made ſeveral trials on board a galley, the ſwiftneſs of which was compared with that of another galley, equipped as uſual. M. *De Chazelles*, a member of the *Royal Academy of Sciences* and engineer of the king's galleys, had orders to make his obſervations and ſend them to court; a copy of his report, is as follows.

An experiment of the ſwiftneſs of a galley, with perpendicular revolving oars, invented by M. *Du Quet*, compared with that of a common galley, made at *Marſeilles*, *&c.*

At 10^h 3′ in the morning, the *ſuperbe* galley quitted her ſtation over againſt the *Auguſtins*, in order to fall down to the *chain*.

At 10^h 11′ ſhe came to the *chain*.

At 10^h 6′ the machine galley quitted her ſtation at the innermoſt part of the harbour; ſhe had three machines on each ſide.

10^h 13′ ſhe came to the *chain*.

10^h 19′ the two gallies abreaſt. Both row with their whole crews.

10^h

10h 25'. The *ſuperbe* paſſes; and then rows only with the hinder part of her crew.

10 27. The machine galley paſſes.

10 28. Both row with their whole crews.

10 30. The *ſuperbe* galley paſſes; and then rows only with the fore-part of her crew.

10 32. The machine galley paſſes; upon which the *ſuperbe* galley claps on more oars, till ſuch time as ſhe has acquired the ſame velocity with the machine galley; and it appeared, that with ſeven or eight oars leſs than her complement on each ſide, ſhe kept up with the machine galley, making about 200 rowers, which was the number of the machine galley's crew: There was a little wind a-head, which retarded the *ſuperbe* ſomething more than it did the machine galley; becauſe the *ſuperbe* had her maſts and yard-arms ſtanding, and the other not.

10 43. Came to the moorings of the iſles. The ſign given for turning.

10 47. The *ſuperbe* was come about. It appeared that the machine galley was conſiderably quicker in turning than the *ſuperbe*.

10 30. They came again into port.

By this it appears, that the machine galley has a conſiderable advantage over the common one, in quitting her ſtation and acquiring her firſt motion: For, in ſeven minutes ſhe ran the whole length of the port; having quitted her ſtation by means of her oars, without towing herſelf by her moorings; which is what another galley would not have effected but very ſlowly: And the *ſuperbe* galley after ſhe had moved from her ſtation, was eight minutes in going a leſs diſtance than the length of the port.

But if we conſider that the experiment was made without the harbour, it ſeems to prove that the common galley had the advantage over the machine galley, tho' the number of hands be equal: For, with eight oars leſs than her complement on each ſide, ſhe kept up with the machine galley, notwithſtanding the greater reſiſtance of the wind againſt her maſts: However, if we conſider, that the crew of the *ſuperbe* was a great deal better than that of the machine galley; that the *ſuperbe* is acknowledged to be one of the beſt ſailors the king has; whereas that which had the machines, is an old decayed galley, and reckoned a very bad ſailor; beſides, that the crew of the *ſuperbe* are much better acquainted with the common oar,

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than the others are with the new way of rowing; and that in the common galley there is no improvement to be made, either with reſpect to the proportion of the oars, their length, the breadth of the pallets, the height of the point of reſt, *&c.* or with reſpect to the conſtruction of the veſſel; whereas in the machine there are ſeveral things to be improved and alter'd in the oars, the hand-ſpikes, and in diſpoſing the men to the beſt advantage.

Theſe things conſidered, it ſeems reaſonable to believe, that a veſſel with the machines might go faſter than one with the common oars; becauſe the loſs of time is avoided, which happens in the ordinary way of rowing.

This experiment, however defective it be, for the above-mentioned reaſons, will prove, that the velocity is greater in this way of rowing than in the other, when the circumſtances on both ſides are equal: For, by his journal, M. *De Chazelles* finds, that the *Patronne*, in company with 14 other galleys, left the port of *Marſeilles* at 50 min. after 3, and rowing all in a calm, came to the iſles at 4^h 23′ which made 33′ in going from the *chain* to the iſles. But the machine galley made the ſame way with 200 men, in 30′, having left the *chain* at 10^h 13′; and arrived at the iſles at 10^h 43′, tho' there was ſome wind a-head.

This experiment ſhews, that the *impetus* does not depend on the number of oars, but the number of men.

A veſſel charged with revolving oars will go as faſt in a calm with 100 men, as it would do if tow'd by a galley of 200 men; becauſe there will be one galley leſs to draw along.

Another memoir of M. *De Chazelles*, concerning the uſefullneſs of perpendicular revolving oars, invented by M. *Du Quet*, is as follows.

The experiment made with the new machine, tho' defective by reaſon of the difference there was with reſpect both to the crew and the veſſels, does ſtill leave room to expect a conſiderable advantage from this invention, in giving the ſhip way: For, tho' the common galley ſhould keep up with the machine galley at their firſt ſetting out with equal number of hands; it is evident, the machine galley will get the better at long run; when the others crew are ſo fatigued, as to be obliged to row by turns. For here the men will hold out a longer time; their action not being ſo great nor ſo violent: Beſides, having only 200 men employed, and being equally mann'd with the other galley, freſh hands may be ſupplied, and ſo they will continue

to

to go at the ſame rate: For, in caſe of need the marines may be employed in this ſervice; which they will perform with as little trouble, as they work at the capſtane.

The reaſon of this encreaſe of velocity plainly appears, if we conſider the difference between the common way of rowing and that by perpendicular oars: The laſt is performed by an uninterrupted application of force, in the ſame direction; the other acts by jerks: And of the three parts of action that are employed, in order to give the ſtrokes; one in raiſing the oar out of the water, the ſecond in advancing the hands forwards, and the third in preſſing againſt the water; only the laſt turns to account; and that ſtill loſes ſomething of its efficacy; for, the crew by their falling back all together, make the veſſel plunge and render its motion oblique, which contributes very much to its decay.

Theſe are not the only defects of the common oars: For, in order to augment their force, their number is to be increaſed; and conſequently, the veſſel muſt have a greater length; by which means it is rendered weaker and leſs able to reſiſt the force of the ſea: Beſides, the veſſel muſt be low built and uncovered (and ſo more expoſed to the beating in of the waves) by reaſon they are obliged to proportion the length of the oar to the ſtrength and ſize of the men: And tho' the crew ſhould be under ſome cover, as they are in a galeaſs; an opening muſt be left for the oars to play, by which the waves may beat in.

Both theſe inconveniencies are avoided by the perpendicular oars; becauſe the addition of force may be obtained; by only applying more hands to the machine; ſo that with two or three machines on a ſide, there will be more or leſs force, in proportion to the number of men employed, and the length of the veſſel may be leſſened at diſcretion: And to guard againſt the ſea, another deck may be made, ſhut cloſe on all ſides, even where the axis of the machine paſſes thro'.

The chief objections againſt this invention ſeem ſufficiently obviated by M. *Du Quet*'s memoir: But, tho' the whole of what is objected ſhould, indeed, prove, that a veſſel made for ſailing, as the common galley, would be incumbred with the machines in ſuch a manner, as to make the uſe of ſails impracticable; yet if it ſtill hold true, that ſhe will move faſter, as appears both by reaſon and fact, it muſt be allowed, that a veſſel might be ſo commodiouſly conſtructed to carry theſe machines,

machines, as to go as faſt as a galley in a calm, and better endure the weather when under ſail.

Such a veſſel would have ſeveral advantages over a galley both in ſailing and in fighting; not to mention the conveniencies of lodging the crew: She may put off to ſea any where, and thereby avoid the dangers attending the coaſt-winds, which galleys find to be a-head as ſoon as they have doubled certain capes; and ſo they find themſelves between two winds, which there would be no danger of, farther out at ſea. With reſpect to fighting ſhe may mount cannon fore and aft and on each ſide; and even mortar-pieces. In time of battle ſhe would be of conſiderable uſe; for, ſhe would take and maintain her poſt without aſſiſtance, either at the head or the rear of the enemy's line and there make uſe of her bombs: Beſides the advantage of towing off other veſſels from their danger in a calm, and of boarding or making off from the enemy: And this holds in ſhips of any rate; provided the length of the oars, the breadth of the pallets and the ſtrength of the hand-ſpikes be proportionable: And the moving force will always be in proportion to the ſtrength and number of the men employed, and not to the number of machines, as in the common oars, which likewiſe are impracticable in ſhips above the fourth rate, by reaſon of their great length, which will be diſproportionate to the ordinary bulk of a man.

By this means the crew will be free from the fatigue of towing, and the veſſel will move incomparably faſter than if it was tow'd; becauſe the chaloups which tow are ſubject to the inconveniencies of the common way of towing, by loſing $\frac{2}{3}$ of the time; and beſides, they cannot act all together: And the veſſel that is tow'd, pulling them back after the oar has made its ſtroke, they have ſo much of the ſpace to regain by the next ſtroke: Beſides, the cable by which they tow, ſinking into the water by its own gravity the reſiſtance the water makes to its return, is to be overbalanced; all which circumſtances together conſiderably diminiſh the towing force.

Beſides, this invention is not ſuch as is deſtructive to mankind, and becomes uſeleſs to the nation that firſt puts it in practice, when generally known; on the contrary it may be greatly advantageous to the inventors at the beginning and every where ſerviceable on ſeveral occaſions, when it is put in practice by thoſe who uſe the ſea.

M. *De*

M. *De Chazelles* might have added, that the chaloups that tow, are in close fight liable to be sunk by the enemy's cannon and are exposed to the waves, by their having so little height above water.

The chief advantage, and which includes all the rest, in boarding or getting clear of the enemy, by means of the new oars, is, that let a vessel crowd as much sail as possible, the perpendicular oars are always capable of increasing her swiftness; because the rowers have only a motion of three foot to make one way; and as much the contrary way, in order to make the oars describe 54 foot space in the water; and that motion of six foot might be performed in two seconds of time, if the oars met with no resistance; consequently, the vessel must run 54 foot in two seconds, that is, about six leagues an hour, before those revolving oars be unserviceable; for, then the vessel would go as fast as the oars could possibly move with a diameter of 18 foot; and if it was necessary to make them move faster, it is only lengthening out their diameter; and they would move so much the faster, without obliging the rowers to increase their own motion.

M. *Arnoult* was ordered to examine the new oars; and he made his report to the court, that the officers of the galleys found, that they interfered with the use of the sails in a galley; but that they might be of use in other vessels and bomb-ketches; in consequence of which M. *Du Quet* was sent to *Toulon* to make the experiment on board a bomb-ketch.

At the time when the experiment was made, M. *De Vauvre* and the officers of the marine were at sea, and only some officers of the port were present, who sent a verbal process to the court, that was given to a general officer to examine and make a report of it; the result of which was, that this invention ought to be put in practice.

An Account of the stocking of the River Mene *with* Oisters; *by Mr.* Rowlands. Phil. Trans. N° 369. p. 250.

THE river of *Mene*, that divides *Anglesey* from *Carnarvonshire*, has at present, *viz.* in 1721, the bottom of its channel for some miles in length, all bedded with good oisters, in such plenty, that in the season, several boats are daily employed to dredge them up; and have done so these 8 or 9 years last past: What is observable is, that about 24 years ago, there were none to be found on that bottom; but that a Gentleman about that time, caused 3 or 400 large oisters to be

dropp'd into the channel, juſt under his land; from the ſpat or ſeed of which, it is very probable (the flux and reflux of the tides diſperſing it) all the bottom at length, where ſmall ſtones and a large clutch received the ſperm, became covered with oiſters: And what favours this conjecture, *viz.* that they are a brood of oiſters begun at that time is, that at the firſt finding, they appeared young and ſmall, but have ſince yearly increaſed in bulk and plenty, tho' vaſt quantities of them have been taken up.

The Longitude of Buenos Aires, *determined from an Obſervation made there; by* F. Feuillèe; *by Dr.* Halley. Phil. Tranſ. N° 370. p. 2.

DR. *Halley* had, as occaſion offered, collected ſuch celeſtial obſervations, as might be of uſe to determine the longitudes of places on the ſea-coaſt of the world, in order to get as near as poſſible the outline or true figure of the earth, without which our geography of the inlands muſt neceſſarily be very inſufficient: The Memoirs of the *Royal Academy* of *Paris* afford a good number of obſervations of this kind, and among the reſt there is one made at *Buenos Aires* on the river of *Plate* in *South America* by F. *Feuillèe* in his voyage to *Peru*; who, in the Memoirs for the year 1711 is ſaid to have obſerved at that place on the 19. of *Auguſt* 1708, the immerſion of the ſtar in the ſouthern foot of *Virgo* (marked λ by *Bayer*) behind the obſcure limb of the moon: Dr. *Halley*, being deſirous to ſee what longitude might be deduced from this obſervation, ſoon found that both the day and ſtar were miſtaken: For, that λ of *Virgo* was then nearly in 3° of *Scorpio*, and that the moon would not be there till the next day, *viz.* *Auguſt* 20; and the Lat. of λ being about ½ a degree north, the moon at that Long. would be about 3° more ſoutherly than the ſtar; and conſequently, far from eclipſing it: For, that at that time the deſcending node was in the very beginning of *Libra*. Hence he concluded, it muſt be ſome other ſtar F. *Feuillèe* obſerved eclipſed by the moon: The day was certainly the 20. and not the 19. of *Auguſt*, as was evident by the place of the moon; but as to the ſtar, it was neither in the *Tychonic* catalogue nor in that more copious one of Mr. *Flamſteed*; but turning over *Hevelius*'s catalogue, the Dr. found a ſtar whoſe ſituation agreed with the obſervation, and was undoubtedly the ſtar, that was obſerved to immerge behind the moon: The place M. *Hevelius* gives it, allowing the preceſſion

preceſſion of the equinox, was then 1° 58′ 40″ of *Scorpio*, with 2° 51′ and ¼ S. Lat. In order to aſcertain the place of the ſtar, the Dr. on the 21. and 24. of *December* 1721, got ſuch obſervations by help of the circumjacent ſtars, that he was aſſured, the place of the ſtar (which is a fair ſtar of the fifth magnitude) was at that time in 1° 58′ 40″ of *Scorpio* with 2° 54′ and ⅓ S. Lat. being upwards of 2′ in Long. and 3′ in Lat. more than *Hevelius* makes it. The hour of this occultation is ſet down preciſely 7ʰ 5′ 38″ at *Buenos Aires*, the Lat. of the place being 34° 35′ ſouth: Whence the altitude of the moon at that place was then 42° 48′ and the parallactic angle 76° 38′ and the parallax in Long. 40′ 11″ to the weſt, and in Lat. 9′ 33″ to the north. So that the moon's obſerved place, corrected by the parallax, was in 2° 28′ 4″ of *Scorpio* with 2° 52′ and ½ S. Lat. To this place (by the *calculus* of thoſe numbers the Dr. fitted to Sir *Iſaac Newton*'s theory of the moon) the moon will be found to have arrived *Auguſt* ²⁰⁄₂₀ at 10ʰ 57′ 36″ apparent time at *London*. But at *Buenos Aires* it was then computed but 7ʰ 5′ 38″ whence the difference of longitude reſulting from this obſervation is 3ʰ 52′ or 58 degrees, by how much *Buenos Aires* is more weſterly than *London*. The Dr. twice repeated the calculation to avoid error; and by comparing his chart of the variation with the Long. thus found, it appears, that in this caſe a ſhip at ſea uſing thoſe tables and that chart, would by an obſervation of this occultation have fallen with greater exactneſs on the coaſt of *America* than can be pretended to be done by any reckoning.

A Deſcription of an Engine *to raiſe Water by the help of* Quickſilver, *invented by Mr.* Joſhua Haſkins *and improved by Dr.* Deſaguliers. Phil. Tranſ. N° 370. p. 5.

MR. *Haskins* finding, that all hydraulic engines, working with pumps, loſe a great deal of water (always giving leſs than the number of ſtrokes ſhould give according to the contents of the barrels) and that when the piſtons are new leathered to prevent that loſs, the friction is much increaſed and the engines are ſubject to jerks, which in large works often diſorder an engine for a great while, by breaking ſome of the parts; contrived a new way of raiſing water without any friction of ſolids; making uſe of quickſilver inſtead of leather, to keep the air or water from ſlipping by the ſides of the piſtons in the barrels where they work; hoping thereby to prevent all the aboveſaid inconveniencies; and alſo to have

water-engines leſs liable to be out of order than any hitherto made.

The firſt experiment Mr. *Haskins* made with an engine ſet up at Dr. *Deſaguliers*'s houſe, which the Dr. repeated before the *Royal Society* in a model; but by the bad contrivance of the parts, it did not raiſe near the quantity of water the invention is capable of; the deſcription of the ſaid engine is as follows,

dddd Fig. 1. Plate V. repreſents a *lignum vitæ* plug or piſton (which Mr. *Haskins* called a plunger) about ſix foot long, made heavy enough with lead at top to ſink into mercury, which is previouſly poured into the barrel D 1 D 2 up to *mm*; the chain E 1 E 2, joined to the piſton and the power that moves it, being let down till the piſton comes to D 2, the mercury riſes to the ſame height in the barrel and in the receiver R (which it fills) namely to *nn*, as appears in the Fig. then drawing up the piſton till its bottom is come to *mm*, the mercury coming out of the receiver down to *oo* makes a *vacuum* and the weight of the atmoſphere cauſes the water to riſe up thro' the ſucking pipe A 1 A 2 and the valve V into the receiver, where the mercury was before. Upon letting down the piſton again, the mercury riſes into the receiver and drives up the water thro' the elbow B, the forcing valve *u*, and ſo up the forcing pipe *a* 2 *a* 1: But when once the forcing pipe (which here was 46 foot high) is full, before any mercury can enter into the receiver and force any water out at the top of the pipe *a* 1, the mercury between the piſton and barrel muſt riſe up to *qq* near 3 foot and $\frac{1}{2}$ above the bottom of the receiver, and as it continues to riſe up to *pp*, the water is thrown out with a velocity, proportionable to the height that the mercury is raiſed above the 14th part of the height of the water. Now tho' the friction of ſolids is here avoided, it is plain, that the mercury muſt move from *mm* to *qq* without raiſing any water, and that it can only force in going from *qq* to *pp* and only ſuck in falling from *oo* to *mm*; and unleſs the piſton be ſtopped a little while when at loweſt, the water will not have time to run out: So likewiſe the piſton muſt be ſtopped, when at higheſt, that the receiver may have time to fill.

Mr. *Haskins* likewiſe propoſed another way, repreſented in Fig. 2. where the ſame letters denote the ſame parts, only here the barrel is moveable by the two chains E 1 E 2; and inſtead of a ſolid piſton, the hollow cylinder C 1 *cc* is fixed, and the mercury moving up and down in the lower part thereof, ſucks

and

and forces the water thro' the elbow. The figure represents the engine sucking, by means of the mercury hanging from *no* to *mm*, in order to force, before any water can be driven out, the mercury in the inner cylinder must descend from *oo* to *mm* and rise up to *pp* between that cylinder and the barrel; so that here likewise a great deal of time is lost; besides the great quantity of mercury us'd, which is very expensive; because so much mercury is mov'd every stroke, as the water rais'd.

These difficulties very much puzzl'd Mr. *Haskins*, and quite discourag'd some other persons that had got the secret of the invention: But Dr. *Desaguliers* considering the matter a little, tho' he had not time to contrive an engine for it, told him, that a little mercury might be made to raise a large quantity of water, and that there would not be such a loss of time as in his engines. In a little time Mr. *Haskins* found out the contrivance represented in Fig. 4. and afterwards that of Fig. 3. which last was what the Dr. had thought of; and both these were also found out by Mr. *William Ureem*, an excellent mechanic.

In Fig. 3. the barrel is mov'd as in Fig. 2. but the plug *ddd* taking up a great deal of space, there is occasion for no more mercury than what will make a concave cylinder or shell up to *pp* between the barrel D 1 D 2; and the hanging cylin- C 1 C 2 *cc*, when the stroke is made for forcing; and a concave cylinder between the plug and C 1 C 2 *cc*, when the suction is made. The Dr. gave Mr. *Haskins* the proportions for an engine this way, of which he made a draught.

In Fig. 4. the barrel with a third cylinder *dddd* instead of the plug of Fig. 3. is lifted up and down every stroke, and the water passes thro' *ddddd*, the mercury making a shell sometimes between the middle and inner cylinder, as in the suction; and sometimes between the barrel and the middle cylinder, as in the forcing stroke.

Mr. *Haskins* had contriv'd such an engine, as is represented by Fig. 4. having bespoke the several parts before he died, the pieces of which the Dr. afterwards made use of; and now the whole put together with some alterations, form the engine, represented by Fig. 5. as set up at the Dr's house at *Westminster*; and by the force of one man raises a hogshead of water in little more than a minute and a half to the height of 27 foot: All the fault of the engine Fig. 5. is, that the pendulum handle *Ff* is too long, and that the bottom of the middle cylinder

C ſhould be exactly in the middle of the height to which the water is to be rais'd, ſuppoſing there are three copper cylinders as here repreſented; and likewiſe if the barrel D 1 D 2 work'd under the forcing pipe, the lift would be the eaſier: The engine with this little alteration is repreſented in Fig. 6.

The ſucking and forcing pipe and valves are mark'd with the ſame letters, as in the other figures; and the chains E 1 E 2 muſt be ſuppos'd to hang from ſuch pullies, and to be mov'd by ſuch a pendulum as in Fig, 5. The barrel D 1 D 2 (otherways call'd the outer cylinder, and repreſented by the ſame letters in Fig. 7.) has another cylinder within it (call'd the inner cylinder or plug as *d d d d* Fig. 7.) between which two cylinders a certain quantity of mercury is pour'd in, and the hanging cylinder C coming down into the mercury, a ſtroke of 13 inches may be made by the motion of the barrel, which in going down ſucks by cauſing a *vacuum* in C, and in going up forces the water out of the top of the forcing pipe, performing the office of a common piſton; only that inſtead of leather to make it tight to the cylinder C, there is always a thin ſhell of quickſilver, either between the middle cylinder C and the inner one *d d d d*, Fig. 7, as happens when the ſuction is made, or between the middle and outer cylinder, as happens in lifting up the barrel to force: In the ſuction, the mercury is higher in the inner ſhell than in the outer, by a height equal to a little more than $\frac{1}{14}$ part of the height of the barrel above the water to be raiſed; and in forcing, it is higher in the outer ſhell than in the inner by a little more than $\frac{1}{14}$ of the height of the column of water to be forced: And therefore, if the water be not requir'd to be rais'd above 64 foot, the barrel ſhould move ſo, as to make the middle of its ſtroke at the height of 30 foot, or at the middle of the way from the water to be rais'd, to the delivery at top.

Fig. 7. drawn by a larger ſcale, repreſents the three cylinders, which are here made of copper in their juſt proportions; whoſe lengths, diameters within and without, and thickneſs are as follows.

Outer

	Outer cylinder or barrel D 1 D 2	Middle or hanging cylinder, C 1 C 2 *cc* in which the ſtroke is made	Inner cylinder or plug *dddd* clos'd at top by a cap and movingup and down with the barrel to which it is join'd at bottom,
	Inches	Inches	Inches
Length	30	29, 0	31, 2
Diameter within	6, 74	6, 35	6, 03
Thickneſs	0, 10	0, 08	0, 13
Diameter without	6, 24	6, 51	6, 29

Here B B repreſents part of the elbow of Fig. 5. or of the forcing pipe of Fig. 4. But as the ſpaces between the cylinder are ſo ſmall as not to be viſible, even in a large draught made by a ſcale; the Dr. has here given three more draughts of the three cylinders, where the height is agreeable to the ſcale of the ſeventh figure, but the diameters of the middle and inner cylinders are made leſs than they are in the engine, to make the intermediate ſpace (where the mercury riſes and falls) viſible; and the cylinders themſelves are repreſented by ſingle lines.

The quantity of mercury us'd in this engine is 36 pounds and ½, which being pour'd in between the outer and inner cylinder riſes up to the height of 16 inches.

When the barrel is pull'd up (as in Fig. 9) ſo as to have the middle cylinder within an inch of the bottom of the barrel; the mercury on both ſides the middle cylinder will riſe up to the height of 23,1 inches, that is, about two inches below the cup D 1, to the line *qq*.

When the barrel is going down to fill the ſucking pipe and middle cylinder C, the mercury in the inner ſhell will be 25 inches high, and only 13 in the outer ſhell (Fig. 9.) where the ſhaded part repreſents the mercury.

At the end of the ſucking ſtroke the mercury is up to the top of the inner cylinder, and ſcarce an inch in the outer ſhell, Fig. 8.

In

In raiſing the piſton from forcing to ſucking, the firſt 1 inch and $\frac{1}{4}$ drives the mercury out of the inner ſhell and raiſes it in the outer ſhell 13,28 inches.

The depth of an inch of water in the middle cylinder above the inner one or plug is equal to a ſpace in the outer ſhell of 13,28 inches; and $\frac{1}{4}$ of an inch is equal to the ſame height in the inner ſhell.

Therefore when the mercury is equally high in both ſhells, a motion of $\frac{1}{4}$ of an inch of the barrel will charge for ſuction; that is, upon letting down the barrel only $\frac{1}{4}$ of an inch, the preſſure of the atmoſphere in the outer ſhell will raiſe the mercury in the inner one 13,28 inches, at the ſame time that it puſhes up the water from the well 13 foot and $\frac{1}{2}$ high into the ſucking pipe; and when all the pipes are full, if the mercury be equally high in both ſhells, upon raiſing the barrel one inch, the mercury will riſe 13,28 inches in the outer ſhell; which the Dr. calls charging for forcing; becauſe in continuing to raiſe the barrel, the forcing valve immediately riſes and the water comes out at top, during the reſt of the ſtroke, which is 12 inches, and delivers 1,6 gallon of water, wine meaſure.

Fig. 10 repreſents the forcing ſtroke half way up, with the mercury 17 inches in the outer ſhell, four inches in the inner, and the whole ſpace at bottom under the middle cylinder ſeven inches.

From this it appears, that in the whole ſtroke of 13 inches in length, there is only $\frac{1}{4}$ of an inch loſt to charge for ſuction; and in the next ſtroke, which is likewiſe of 13 inches, there is only one inch loſt to charge for forcing: So that in a motion of 26 inches, there is but one inch and $\frac{1}{4}$, or about $\frac{1}{20}$ part ineffectual: But this is owing to the too large ſpace of the outer ſhell, which contains four times more than the inner one, becauſe the cylinders were only hammer'd and not turn'd; for, if the outer ſpace had been no bigger than the inner, then $\frac{1}{4}$ of an inch of the ſtroke would have charged for forcing: So that only $\frac{1}{2}$ an inch in 26 or $\frac{1}{52}$ part of the whole ſtroke would have been ineffectual; and in that caſe $\frac{4}{5}$ of the quantity of mercury, or a little more than 12 pounds would have been ſufficient.

There may ſtill leſs mercury be us'd, if the middle cylinder be made of plate iron, turn'd on the outſide and bor'd within; the outer cylinder bor'd and the inner one turn'd: So that

that if the work be well executed, 8 or 10 pounds of mercury will be ſufficient in this engine; tho' the bore of the middle cylinder or diameter of the column of water, which is rais'd, be of 6,35 inches: If the bore of the ſaid cylinder were but three inches, leſs than three pounds of mercury would be ſufficient; and if there were two barrels, leſs than ſix pounds, in order to keep a conſtant ſtream thro' a pipe of almoſt the ſame diameter. This will very much leſſen the expence of mercury, which would otherways be an objection againſt this engine; and by making the inner and outer cylinder of hard wood, as box-wood or *lignum vitæ*, the coſt of the engine may ſtill be reduced: But if the engine be very large, caſt iron bor'd will be proper for the outer cylinder, and caſt iron turn'd on the outſide for the inner cylinder or plug; and hammer'd iron bor'd and turn'd for the middle cylinder.

The following objection ſeems at firſt to take off the intended advantage of this engine, *viz.* that inſtead of the friction of the leather of a piſton, when we lift up our barrel to force, the reſiſtance the mercury finds to riſe in the outer ſhell, is at leaſt as great as the friction we avoid: Now that reſiſtance is never greater than the weight of a concave cylinder of mercury, whoſe height is the greateſt to which the mercury riſes in the ſaid ſhell, and the baſe is the area of the ſhell itſelf: This weight in the engine is equal to 57,5 pounds; and therefore one would think it greater than the reſiſtance made by the friction of a piſton: But if it be conſider'd, that in the deſcent of the barrel for ſucking, the mercury ſhifts immediately into the inner ſhell, riſing to the ſame height, and ſtill keeping the ſame baſe; the aforeſaid weight of 57,5 pounds helps down the barrel, and facilitates the overcoming the force of the atmoſphere; conſequently the weight of the mercury being balanc'd, is no hindrance, whether you work with a ſingle or double barrel.

There only then remains the hindrance by loſs of time in the beginning of any ſtroke: But the Dr. has ſhewn that to be but $\frac{1}{52}$ part of the ſtroke. He has found, that the beſt engines, now in uſe, generally loſe near $\frac{1}{5}$ of the water, that they ought to give, according to their number of ſtrokes: And Mr. *Henry Beighton*, having a great many times meaſur'd the water, rais'd by engines in mines, found, that ſome engines loſt $\frac{1}{4}$; and none ever loſt leſs than $\frac{1}{5}$ of what they ought to give, according to the number of the ſtrokes in their pumps, whatever auxiliary powers they were mov'd with.

There is another objection, but ſcarce worth notice; and that is, that ſome particles of mercury will mix with the water that is rais'd and make it unwholeſome: But no body, that conſiders ſpecific gravity, will imagine any ſuch thing. However, to ſatisfy ſuch as might ſtill apprehend it; it is to be obſerv'd, that none of the water that is rais'd comes near the mercury: For, in the cylinder C and part of the elbow B Fig. 5, there is always above the mercury a certain quantity of water that riſes and falls with the barrel, and never goes into the forcing pipe. The ſame does likewiſe happen in the engine repreſented by Fig. 6. For, the water having once run into the cylinder C, all that is rais'd afterwards, comes thro' the forcing valve, without coming down to the mercury.

Provided care be taken to make the barrel with its plug tight, this engine will not want repair in a long time, unleſs ſome of the auxiliary powers be out of order, which have no relation to this invention. The numbers given will ſerve to examine the truth of what the Dr. has aſſerted concerning the motion of the mercury: And from them one may make tables to ſerve to proportion theſe engines for raiſing any quantity of water to any height, according to the power to be applied.

An Account of the coming off of the Scapula, *and Head of the* Os Humeri *upon a Mortification; by Mr.* Peter Derante. Phil. Tranſ. N° 370. p. 15.

NOV. 5. 1713. *John Fletcher* on board the *Neptune* of *Liverpool* broke the *radius* and *ulna* of his left arm, and their ends burſt out thro' the ſkin: He was immediately dreſs'd by the ſurgeon of the ſhip with the common aſtringents and bandages: About five or ſix days after, Mr. *Derante* was ſent for to ſee the patient; and upon taking off the dreſſings, he found it black and inſenſible from his fingers to his ſhoulder; and therefore advis'd the extirpation thereof immediately; but his ſurgeon oppos'd it: However Mr. *Derante* ſcarified it in ſeveral places, and very deep, and then applied a warm dreſſing: Next day the ſhip put to ſea, and the patient was ſent to *Waterford* and committed to Mr. *Derante*'s care. As ſoon as he could get his *apparatus* ready, he cut off his arm as high as poſſible; afterwards he cauteriz'd the ſtump, which was perfectly mortified as high as the *acromion*. Next day he perceiv'd the mortification ſpreading towards the lower angle of the *ſcapula*; then he rubb'd the edges of the mortification with arm'd probes, dipp'd in a ſolution of *argent. viv.*

div. in *aq. fort.* which compleatly anſwer'd his intention: For, from that time the mortification ſpread no farther; next dreſſing he ſcarified and cauteriz'd all the mortified part and dreſs'd it *ſecundum artem.* He continued this method for 17 or 18 days; and then the ſloughs began to ſeparate and caſt off daily: Some time after, the *ſcapula* began to part from the *Os humeri* and *clavicula*; and at length came out entirely; the *Os humeri* ſtill adher'd to the *pectoralis* and *latiſſimus dorſi*; but in a little time it alſo ſeparated and came away, without any hemorrhage ſucceeding: He was afterwards oblig'd to cut off part of the *clavicula*, before he could cicatrize the wound, which was ſoon after accompliſh'd.

A great Number of Hydatides *found in the* Abdomen; *by M.* Bartholin Anhorn ab Hartuiſs. Phil. Tranſ. N° 370. p. 17. *Tranſlated from the* Latin.

A Man of 50 years of age, of a cholerico-phlegmatic conſtitution, and ſquare ſtature; not fat but robuſt, of a good appetite, and who went about his ordinary buſineſs very well, and obſerved a pretty good *regimen*, not given to wine or ſpirituous liquors to exceſs, in *Auguſt* 1721 applied to M. *Anhorn* in the caſe of a ſwell'd and tenſe *abdomen*, not unlike an *aſcites*; tho' his urine was laudable, and in a ſufficient quantity, and he had no *œdematous* ſwelling in his legs: his reſpiration was free, tho' his belly increaſed daily in bulk, having begun to ſwell the *June* before; he had the hemorrhoids to a great and ſometimes painful degree; yet without any loſs of ſtrength or appetite.

M. *Anhorn* was at a loſs to determine, whether it were a dry or moiſt tumour: But to ſatisfy the indications, particularly the ſuppreſſion of the hemorrhoids, he adminiſtred chalybeates, mix'd with purgatives, by which the hemorrhoidal flux was immediately promoted, the pain abated and a large quantity of *fœces* with ſome relief to the patient, diſcharged: Afterwards M. *Anhorn*, as *Etmuller* had done in the caſe of a dry dropſy, tho' to no purpoſe, adminiſter'd daily *unc. ſſ. Nitr. præparat.* as alſo *Mercur. Vitæ.* together with the milder and ſtronger diuretics, *&c.*

The patients *abdomen* increaſing very much in bulk, it was neceſſary without farther delay to proceed to the *paracenteſis*, which was perform'd *Jan.* 14. 1722; but the contents being of a glutinous conſiſtence could not be diſcharged by the *cannula*: ſo that an inciſion muſt be made with a lancet; upon which

which there was discharged the first day about six measures of a glutinous matter of a grey dirty colour, thicker than the whites of eggs, together with a large quantity of white globules of different figures, as spherical, triangular, *&c.* of the bigness of filberds, yet without lots of strength: Next day again the incision was enlarged with a lancet, upon which a great number of globules exceeding in bigness pidgeon's eggs, was discharged, together with six measures more of a glutinous matter, and a large piece of a whitish mass resembling a portion of the *omentum*, to which mass several of the said globules adher'd by fibrous filaments.

Upon setting the glutinous matter over the fire, it became hard and white, with a pellicle on it.

And upon pouring on the said matter spirit of vitriol, spirit of *Sal-armoniac*, volatile oil of tartar *per deliquium*, rectified spirits of wine and vinegar, there was nothing other observ'd than a gentle induration thereof, by each of the said liquors, tho' of quite opposite natures.

The globules were of a very white colour, and hard to the touch; upon breaking, there issued out a white liquor, resembling chyle, and included in a membrane or pellicle.

That whitish mass, resembling a portion of the *omentum*, tho' an inch thick, was evidently membranous.

The number of small and large globules was about 7 or 8000.

The 16. day inclusive after the operation, the patient did very well, his belly was soft and smooth, the wound look'd well, his urine, pulse and respiration were laudable; consequently, he had no fever, and he slept sound: For 13 days there was a plentiful discharge of a glutinous inodorous matter, and which at length became serous and in little quantity, but of a strong smell, and turning black a silver catheter put into it, which was ascribed to corrupted hetereogeneous bodies rather than to the *viscera* being affected with a gangrene, because there was no fever. On the 16. day after an injection of a decoction of elecampane, agrimony and mellilot with a tincture of aloes, myrrh and *balsam du commandeur*, a pellicle or a broken globule was discharged, which in its natural state was as big, and probably bigger than a hen's egg.

The patient's appetite and strength were now the chief things to be managed; M. *Anhorn* administer'd vulnerary, alexiteric and comforting medicines, combined together according to the rules of a strengthning diet.

M. *An*

M. *Anhorn* leaves others to diſcuſs this remarkable phænomenon.

An Account of ſome Experiments made with the Bile of Perſons Dead of the Plague *at* Marſeilles, *with what appeared upon the Diſſection of the Bodies; as alſo ſome Experiments made with the Bile of Perſons Dead of other Diſeaſes; by* Dr. Deidier. Phil. Tranſ. N° 370. p. 20.

Exp. 1. THE human bile, taken from the gall-bladder of the bodies of ſuch as died of the plague at *Marſeilles*, has always been found to be of a black and greeniſh colour: Upon mixing ſpirit of vitriol with the ſaid bile, it conſtantly became of a laſting graſs green; and upon mixing therewith oil of tartar *per deliquium*, or its alcalious fixed ſalt, diſſolv'd in a ſufficient quantity of water, it always became very yellow: Theſe two colours green and yellow have continued for whole months. The ſaid bile has become of a black colour, like ink, but it ſoon fades, by pouring ſpirit of nitre thereon.

Exp. 2. The bile taken from the gall-bladder of the bodies of ſuch as died of the plague, having been poured into a wound made on purpoſe in ſeveral dogs, rendered them melancholly, drowſy, without caring to eat: All theſe animals died in three or four days with the eſſential marks of the true plague; as bubo's, carbuncles and gangrenous inflammations in the *viſcera*, in the ſame manner as in the human bodies from which the bile was taken.

Exp. 3. A drachm of the ſaid peſtiferous bile having been mix'd with two ounces of fountain-water, made lukewarm, and injected in the jugular vein of ſeveral dogs, rendered them preſently drowſy and killed them in four hours, with gangrenous inflammations, the heart ſtuffed full of black thick blood, the liver ſwell'd and the gall bladder full of green bile.

Exp. 4. The ſame quantity of bile, injected by the crural vein, in about an hour produced a heavineſs in the dogs; they had ſuch a ſtrong loathing of their food, that they would neither eat or drink after the injection was made; they frequently made water, eſpecially if ſtirred: On the third day there appeared conſiderable tumours under the *axilla* and their thighs, about three inches from the wound; the wound turned to a gangrene, and the animal uſually died on the 4th day, with all the ſigns of the plague.

Exp.

Exp. 5. A dog, belonging to the hoſpital at the *Mail* in *Marſeilles*, who followed the ſurgeons, when they went to dreſs the patients, uſed greedily to ſwallow the corrupted glands and the dreſſings full of *pus*, taken off the plague-ſores; he lick'd up the blood, he found ſpilt on the ground in the infirmary; and this he did for about three months; and was always well, gay, briſk, full of play and familiar with all comers; into the right crural vein of this dog about a drachm of the peſtiferous bile was injected, mixed with two ounces of warm water.

He died the 4th day, as the others did, with a bubo on the wounded thigh; on which there were likewiſe two carbuncles and the wound was gangren'd. What was remarkable in this dog is, that after the injection, both when alive, and after he was opened, he had a very ſtinking ſmell, which was not obſerved in any of the others: Moreover, there was a conſiderable hemorrhage from the wound, the night before he died, having ſtruggled hard to eſcape out of his confinement.

Exp. 6. *May* 2. having injected into the crural vein of a dog about a drachm of human bile, taken from perſons dead of the plague, mixed with two ounces of water, the creature became preſently drowſy, refus'd his food, and died about the 3d or 4th day after the injection, with all the inward and outward ſigns of the plague that the others had.

Exp. 7. *May* 6. the bile of this dog, dead of the plague, being injected by the crural vein into the blood of another dog, he, preſently after the injection, had convulſive motions all over him, that were followed by a lethargic heavineſs: On the ſecond day after the injection, there appeared a carbuncle upon the great pectoral muſcle on the right ſide: The third day there aroſe a conſiderable bubo on the thigh, and the dog died the ſame day. Upon opening the body the fore-part of the breaſt was found all gangrened under the teguments; and in the inner parts, the *viſcera* were full of black clotted blood; as in all the others: The external ſurface of the lungs was all of a purple colour; the heart was ſwelled as big again as uſual, with the four cavities full of black clotted blood. The dog lived three days after the injection without eating or drinking.

May 10. the bile of this ſecond dog was injected into the crural vein of a third dog, who was thereupon ſeiſed with violent convulſions and various convulſive motions for about half a quarter of an hour: When he recovered from theſe, he appeared dull and ſleepy; he vomited with violent ſtrainings; the vomiting

was

was followed with an hick-up: He eat a little boil'd meat, having faſted a good while before the injection was made; but he vomited it up two hours after he had taken it. He died the third day with the ſame ſigns of the plague that the other dogs had.

The ſtate of the bodies of ſuch perſons as died of the plague, from which the bile for the abovementioned experiments was taken, is as follows.

Belefleur a ſoldier, 25 years of age, of a ſtrong robuſt conſtitution, having a flattiſh bubo growing on the hollow of the right groin, died delirious: Upon opening his body his heart was found of an extraordinary bigneſs, ſtuffed with black clotted blood; his lungs covered with a livid purple, and adhering a little to the *pleura*; the liver was twice the natural bigneſs, and ſtuffed with a thick blood; the gall-bladder was full of a black and greeniſh bile; the *dura* and *pia mater* ſeemed by their blackneſs to have been ſeiſed with a gangrenous inflammation; the internal ſubſtance of the brain was ſprinkled over with an infinite number of ſmall livid ſpots.

Mary Piſane, 30 years of age, of a ſanguine habit, had a bubo under her right arm-pit, with a *delirium* that was followed by a mortal ſleepineſs: In opening her body, the lungs were in their natural ſtate; the heart was of a prodigious bigneſs, full of black coagulated blood, with the left auricle livid and gangrenous; the liver, which was much enlarged, was all covered with purple; and the gall-bladder filled with a black and greeniſh bile.

Peter Moulard, of a tender, feeble conſtitution, about 14 years of age, had a bubo under the hollow of the right groin, that was very deep and never came well out: He became delirious, with convulſions, in which he died. Upon opening his body, the heart was found twice its natural bigneſs, containing a black thick blood; his lungs had livid ſpots here and there; the gall-bladder was full of a black and greeniſh bile.

Jean Raynaud, a cook, about 25 years of age, of a melancholly temperament, had the whole habit of his body covered with a purple livid colour, and a bubo under his left arm-pit, he died in a delirious phrenzy. Upon opening his body, two abſceſſes were found, one between the teguments and the left great pectoral muſcle, and the other in the breaſt between the *ſternum* and the *mediaſtinum*; his heart was very large, filled with black thick blood; the right auricle was three inches broad; the left was in its natural ſtate; his lungs were covered with ſmall livid ſpots, remaining ſoft and pliant, without any hardneſs in their

ſubſtance

ſubſtance; the liver was larger and harder than ordinary, and alſo full of livid purple ſpots; and the like were found in the ſubſtance of the brain, all the veſſels of which were filled with black thick blood.

Jaques Audibert, about 35 years of age, of a melancholly complexion, four months after he had been cured of the plague (the ſign of which was a bubo in the fold of the right groin, which came well to a ſuppuration) was attack'd afreſh with three carbuncles; one in the middle of the arm and the other two in the fore-arm: He had but little fever, with ſome little ſickneſs at ſtomach; but a delirium, ſeiſing him of a ſudden, carried him off. Upon opening his body, his heart was found of a prodigious bigneſs; the right auricle being five inches broad and the left three; a ſmall impoſthume was found upon the body of the *aorta*; the lungs were covered with livid ſpots, and the liver appeared gangrenous; the gall-bladder was of a very black colour; the *duodenum* and *rectum* were inflamed.

Venture Cajole, about 40 years of age, of a melancholly temperament, died on the 3d day without any outward eruption, of a violent fever, with a ſleepineſs: Upon opening the body, the *mediaſtinum* was found torn towards the upper part; the *pericardium* was of a livid colour; the heart larger than in its natural ſtate, by the ſwelling of its ventricles, which were full of black thick blood, as in the other ſubjects; the liver was likewiſe very large and of a livid colour, with a carbuncular puſtule on the ſide of the gall-bladder, which was filled with very black bile.

Margueritte Bachaire, 18 years of age, of a lively vigorous conſtitution, having two carbuncular puſtules on the middle and inſide of the thigh, with a ſharp pain in the head, died delirious. Upon opening her head the integuments of the brain were found of a blackiſh red; the cortical part of a livid red and a few black ſpots upon the medullary part; the heart was of a prodigious ſize, full of thick black blood; the liver was likewiſe very large, and the gall-bladder very full of a black and green bile; there were ſeveral livid ſpots upon the ſurface of the inteſtines.

Louiſe Belingere, 20 years of age, having a bubo in each fold of the groin, died very ſuddenly without any dangerous ſymptoms. Upon opening her body, the heart was found all covered with a livid purple, and much larger than natural, filled with thick black blood, with a *polypus* in each ventricle; the lungs were in their natural ſtate; the liver was of a prodigious bigneſs; and the gall-bladder full of bile of a deep green colour.

Rampeau,

Rampeau, a peaſant, about 20 years of age, of a ſanguine robuſt conſtitution, having had a carbuncular parotid for the ſpace of eight days, accompanied with a burning fever, was carried to the hoſpital the 2d of *May*, where he died the 5th. The external part of the left ſide of the lungs was found covered with a livid purple; the heart was twice its natural bigneſs, having ſcarce any blood in the ventricles, each of whoſe cavities was filled with a large *polypus*, that on the right ſide having dilated the auricle to the breadth of four inches; the liver alſo was larger than ordinary, and the gall-bladder full of a black and green bile.

The abovementioned experiments and the opening the bodies were made in the hoſpital at the *Mall*, in the apothecary's ſhop of the reformed Fathers of *Marſeilles*, during the months of *February*, *March*, *April* and *May*.

The following experiments were made at *Montpelier*, in the hoſpital of St. *Eloy*, during the months of *September*, *October* and *November*.

A ſoldier, of about 20 and 25 years of age, of a lively briſk temper, being ſick in the hoſpital of St. *Eloy* of a common malignant fever, died about the 15. day by a defluxion upon his breaſt: His lungs were found very much inflated, filling all the cavity of the breaſt, and adhering to the *pleura*: Upon obſerving the bile in the gall-bladder of a bright graſs-green colour, it was reſerved for the following experiments.

This bile, mixed with four ounces of warm water, was partly injected into the jugular vein of a dog, and a compreſs, ſoak'd in the reſt of the liquor, was applied to the wound. The dog thereupon appeared heavy and ſleepy, and would neither eat nor drink; the third day he eat and drank freely. The compreſs falling off the fourth day, the wound was diminiſhed one half and healed up by degrees, and the dog grew perfectly well.

Exp. 8. A peaſant of about 50 or 60 years old, of a melancholly temperament, had been near a month in the hoſpital with an ordinary malignant fever, being alternately delirious and ſleepy: After his death, the bile was found exceeding thick, and black as ink, and in great quantity: About a drachm was put into a wound made for that purpoſe on the outer part of the right thigh of a dog, thruſting in pledgets, dipt in the ſaid bile diluted, into the wound: No change appeared in the dog: He was made to ſwallow ſome of the ſaid bile, without loſing his appetite; and ſeeing he was like to be well, the wound healed up of itſelf in 15 days, only by the dog's licking it.

Exp. 9. A pledget, ſoak'd in as much of this bile diluted, as it could take up, was applied to a freſh wound, made in the inſide of the right thigh; the pledget was faſtened within the ſkin by ſome needles: This application produced no conſiderable alteration in the dog; he neither appeared ſleepy nor without appetite; but lick'd his ſore readily enough; and after the pledget was fallen off, the wound healed up as in the foregoing experiment.

Exp. 10. About a drachm of the ſame black bile, taken from the ſame body and mixed with warm water, was injected into the jugular vein of another dog, who was not incommoded thereby, but as briſk as before the injection, only he appeared very thirſty and lapped greedily: Next morning the wound was black and dry, and the dog, becoming ſurly, bit one of the aſſiſtants: The two ligatures made for the injection were taken off, without obſerving any blood to run out: A doſſil was applied, charged with the ordinary digeſtive and kept on by a bandage; and about four hours after the dreſſing the dog was found dead, having lived 23 hours after the injection: Upon opening him, his heart beat ſtill with violence, and upon the ceaſing of the pulſation there was no blood either in the ventricles or auricles. This liquor, crowded together in the large veſſels, appeared of a lively red and very fluid, without any of thoſe concretions, conſtantly obſerved in all the bodies, that died of the plague: Here there appeared neither internal nor external ſigns of the plague.

Exp. 11. An inhabitant of *Montpelier*, about 30 or 35 years of age, very fat and robuſt, of a ſanguine complexion, having had a fall upon the pavement, received a ſimple wound on the upper part of his fore-head on the right ſide: This being neglected, brought on an *eryſipelas* all over his face, which was accompanied with a ſwelling of the left parotid: This appeared and diſappeared thrice from morning to night. The *eryſipelas* came ſuddenly on; the patient grew delirious and died after 15 or 20 days illneſs, reckoning from the fall. Upon opening his body, a quantity of water was found between the ſcull and the *dura mater*: The brain, which was firmer than ordinary, was ſomewhat red, and part of the *pia mater*, covering the hinder part of that *viſcus*, appeared inflamed. There was about ½ a *ſeptier* of water of a yellowiſh colour, ſhed in the cavity of the breaſt; the great right lobe of the lungs was ſomewhat hard on its upper part; the heart had a *polypous* concretion in each ventricle: Two *French* pints of limpid water were likewiſe found

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in the lower belly: All the fat of this ſubject was yellow: The liver appeared ſomewhat ſwell'd, and the gall-bladder almoſt empty, not having above two drachms of yellow bile therein. The bile of this ſubject, mixed with two ounces of warm water, was injected into the crural vein of a dog, who eat and drank heartily after the injection, and did not appear at all incommoded: The wound bleeding much, it was filled with reſtringent powders, kept in by a pledget and a convenient bandage: Twenty four hours after the dreſſing was taken off, the wound appeared black and dry: The dog licking it, it ſuppurated the next day, and afterwards became red and well coloured; and the wound was leſſened one half in eight days, during which time the dog appeared in perfect health.

Exp. 12. Eight days after the foregoing experiment, the dog that was the ſubject of it was killed by about ½ a drachm of powder of *Hungarian vitriol*, diſſolved in a ſpoonful of warm water, and injected into the jugular vein: The creature died on the ſpot with univerſal convulſions: His heart was found full of grumous blood, reduced to a kind of thick pap, but without any clotts: The bile of this dog was yellow and in ſmall quantity: Not being able to inject it into the crural vein of another dog, becauſe the vein was too ſmall, two compreſſes were dipp'd in this bile, which were applied and kept under his ſkin by two wounds, made on purpoſe; but without any remarkable change. In theſe two dogs were obſerved no ſigns of the plague either internal or external.

The Method of inoculating the Small-pox *in* New England; *by Mr.* Newman. Phil. Tranſ. N° 370. p. 33.

1. A Couple of inciſions is uſually made in the arms, where iſſues are made, but ſomewhat larger than for them; ſometimes in one arm and one leg. 2. Into theſe bits of lint are put (the patient at the ſame time turning his face another way, and guarding his noſtrils) dipt in ſome of the variolous matter, taken in a phial, from the puſtules of one that has the ſmall-pox of the more laudable ſort, and juſt turning upon him, and ſo covered down with a plaiſter of *diachylon*. 3. The variolous matter, from ſuch as have the inoculated ſmall-pox, is altogether as agreeable and effectual, as any other; as alſo that, taken from ſuch as have the confluent ſort. 4. In 24 hours after, the lint is taken off, and the ſores are dreſs'd once or twice every 24 hours, with warmed cabbage leaves. 5. The patient continues to go about his affairs, as at other times, only he muſt not expoſe himſelf

ſelf to the injuries of the weather, if any ways tempeſtuous. 6. About the 7th day the patient feels the uſual ſymptoms of the ſmall-pox coming upon him; and he is now managed, as in a common putrid fever; if he cannot hold up, he goes to bed; if his head ach too much, the common poultice is put to his feet; if he be very ſick at the ſtomach, he has a gentle vomit; nay, commonly theſe things are almoſt done of courſe, whether the patient want them or not, and the ſooner, it is thought the better; if the fever be too high, in ſome conſtitutions, a little blood is taken away; and finally to haſten the eruption, a couple of bliſters is applied. 7. On or about the third day from the decumbiture the eruption begins: The number of the puſtules is not alike in all; in ſome they are very few; in others they amount to 100; nay, in many to ſeveral hundreds; and frequently to more than by the accounts from the *Levant* is uſual there. 8. After the eruption, all illneſs vaniſhes; except, perhaps, a little of the vapours in thoſe that are troubled with them: And there is nothing other neceſſary than to keep warm, drink proper teas, eat gruel, milk pottage, panada, bread, butter and almoſt any thing equally ſimple and innocent. 9. Ordinarily the patient ſits up every day, entertains his friends, nay, ventures on a glaſs of wine; if he be too intent upon hard reading and ſtudy, he muſt be taken off it. 10. Sometimes tho' the patient be eaſy enough on other accounts, yet he cannot ſleep for ſeveral nights together: In this caſe no anodynes or opiates are given; becauſe it is found, that ſuch, as have taken theſe things in the ſmall-pox, are generally peſter'd with biles after their recovery; and ſo they are omitted, ſleep coming of itſelf, as their ſtrength comes on. 11. On the 7th day the puſtules uſually come to maturity; and ſoon after go off, as thoſe of the ſmall-pox in the diſtinct ſort. 12. The patient gets abroad quickly, and is moſt ſenſibly ſtronger, and in better health than he was before. The tranſplantation has been made on women in child-bed, eight or nine days after their delivery; and they have got earlier out of child-bed, and been in better circumſtances than ever in their lives before: Thoſe that have had ugly ulcers, long running upon them, have had them healed up: Some very feeble, crazy, conſumptive people have, upon this tranſplantation, grown hearty and got rid of their former maladies. 13. The ſores of the inciſion ſeem to dry a little in three or four days of the feveriſh preparation for eruption; after this there is a plentiful diſcharge at them: The diſcharge may continue a little while after the patient is quite well in other reſpects; but the ſores will ſoon enough dry up of themſelves;

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ut the later, it is thought the better. If they happen to be inflamed, or otherways troublesome, they are presently managed as common sores.

An Account of the Inoculation of the Small-pox *at* Halifax *in* Yorkshire; *by Dr.* Nettleton. Phil. Trans. N° 370. p. 35.

DR. *Nettleton*, having too often found, how little the assistance of art could avail in several cases of the small-pox, was induced to try the method of insition or inoculation, which came so well recommended by several physicians from *Turkey*, and which had also been practised in *London*; the following is an account of its success and of every thing either done or observed in it at *Halifax* in *Yorkshire*.

In *December* 1721, the Dr. first began to put this method in practice, and finding it to succeed beyond expectation in the first instance, he was encouraged to repeat it upon some others; and afterwards several people, seeing with how much ease those got over the distemper, were desirous to have the same done either on themselves or their children; so that there were upwards of 40, who received the small-pox by insition, and who all got well and were afterwards in perfect health, only one patient dying.

What was done by way of preparation, was chiefly purging with rhubarb for children; and sometimes vomiting or bleeding for grown persons; and several had no preparation at all. But the Dr. always found, as far as he was able to judge, that those, whose bodies were well prepared by such proper methods, as their different ages or constitutions seemed to require, had more favourable symptoms than others in like circumstances, where that was omitted.

The method the Dr. always took in the operation, was to make two incisions; one in the arm and another in the opposite leg: It is not material as to raising the distemper, whether the incisions be large or small, but he commonly found, that when they were made pretty large, the quantity of matter discharged afterwards at those places was larger; and the more plentiful that discharge, the more easy the rest of the symptoms generally are, and by this means they are also best secured from any inconveniency, which might follow, after the small-pox are gone off.

At first the Dr. collected some of the matter from the pustules of one, who had the small-pox of the natural sort, into a shell or phial, and infused two or three drops of it into the wound; but finding it to be very troublesome and difficult to get any quantity

of

of the matter, and likewiſe obſerving, that the ſmalleſt quantity imaginable will be ſufficient for the purpoſe, he commonly took ſmall pledgets of cotton and ripping the puſtules, when ripe, with the point of a lancet, he rolled the pledgets over them, till they imbib'd ſome of the moiſture; one of theſe he put on each wound, and cover'd it with any common plaiſter till next day, when he commonly took away both the cotton and the plaiſter, leaving the wound to itſelf, only covering it with a ſlight linen roller to defend it from the air: He has ſometimes rubb'd the pledget only once over the wound, without binding it on, which he found to anſwer the end as well; and from ſome other obſervations he made, he has been ſurpris'd to ſee the ſmall pox produced this way, when he was very well aſſur'd, the quantity of matter, receiv'd into the veſſels, could not amount to the hundredth part of a grain.

The perſons inoculated were not confined to any regimen, but only to be kept moderately warm; and thoſe, who were grown up, to live very temperate and regular, to keep their minds eaſy and compos'd, and to uſe proper means to drive away all fear and concern: Some have been oblig'd from the time of the inſition to abſtain from fleſh and all ſtrong liquors; but he afterwards found, that the inſition did not proceed ſo well, when they were obliged to live too low. Perhaps in warmer climates where they are not ſo much accuſtom'd to live on meat, ſuch abſtinence may be neceſſary; but here he found it beſt to let them eat or drink as uſual, tho' ſomething more ſparingly, till the fever begin to riſe; and then, but not before, ſuch a regimen, as is uſual in like caſes, is enjoin'd.

The firſt thing that occur'd after the inſition, was the inflammation of the wounds, which commonly happened about the fourth day, when they began to appear very red round about, and to grow a little ſore and painful; in about two days more they began to digeſt and run: In ſome they begin to run ſooner, and the quantity diſcharged is much greater than in others. The Dr. generally found, that in thoſe, who diſcharged moſt this way, the fever was more ſlight and the ſmall-pox fewer, tho' he has known ſome do very well, when theſe places have only appear'd very red; but have ſcarce run anything at all, as it uſually happens, when the inciſion is made ſo ſuperficial, as not to cut thro' the ſkin.

About the ſeventh day the ſymptoms of the fever begin to come on, which are the very ſame, that are always obſerv'd in the ſmall-pox of the diſtinct kind, in the natural way: A quick pulſe, great heat and thirſt, pain in the head and back, and about the region of the ſtomach, vomiting, doſedneſs, ſtartings, and ſometimes convulſions; all were not ſeiz'd with all theſe ſymptoms, nor in the ſame degree or continuance; ſome began on the ſeventh day and continued ill without any remiſſion, till after the eleventh; and many not till the eighth or ninth day; and in theſe the fever was more moderate with conſiderable intermiſſions; and ſome have ſcarce had any illneſs at all: During all this time the places of inciſion continued to be very ſore, and ſwell very much; ſo as to appear very large and deep, and diſcharge a great deal of matter.

On the tenth day the ſmall-pox moſt commonly appear'd, ſometimes on the ninth, and ſometimes not till the 11th; but the Dr. never found, that any difference of age, conſtitution or any other cauſe, ever made them vary above one day from the 10th. The number of puſtules was very different; in ſome not above 10 or 20; moſt frequently from 50 to 200; and ſome have had more than could well be reckoned, but never of the confluent ſort: Their appearances was the ſame with thoſe of the diſtinct kind; they commonly came out very round and florid; and many times roſe as large as any he obſerv'd of the natural ſort, going off with a yellow cruſt or ſcab as uſual; tho' it ſometimes happened, eſpecially when the ſores diſcharged a very large quantity of matter, that they were both few in number and did not riſe to any bulk; but having made their appearance for four or five days, they waſted inſenſibly away.

After the ſmall pox come out, the feveriſh ſymptoms gradually abate, and when the eruption is compleated, they uſually ceaſe without any ſecond fever, or any farther trouble in any reſpect.

While the puſtules were riſing, and for ſome time after they were gone, the ſores continued to ſwell and to run very much; the longer they do ſo the better: But they never fail'd to heal up of themſelves after a certain time.

The Dr. rarely ſaw occaſion for any medicines in the courſe of the diſtemper, only ſometimes when the ſymptoms ran very high, he gave a gentle anodyne, to be repeated, as occaſion ſhould require; and once or twice he thought it neceſſary to bliſter and to uſe ſuch medicines, as are found to be moſt ſer-

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viceable in the ſmall-pox of the natural ſort. After the puſtules are gone off, the patients have always been purged twice or thrice and ſometimes let blood, which is all that has been uſually done. But tho' the practice may ſeem to be very eaſy, yet it is an affair of ſuch a nature, as to require the utmoſt care; and never to be undertaken without the advice of phyſicians to direct a proper method of preparation before the inſition is made, as well as a juſt regimen afterwards; to watch every ſymptom, and lend nature all proper aſſiſtance, whenever it ſhall be requiſite. Where this is done it will ſeldom fail of being attended with happy ſucceſs.

In one inſtance or two it has happened, that the ſymptoms in the diſtemper have been worſe than uſual; and ſome few after the ſmall pox were gone off, have been ſubject to other indiſpoſitions; a particular account of which is, as follows.

A boy of *Halifax*, about a year and a half old, that was inoculated, began to be ill on the eighth day, and had a briſk fever for two days; then the puſtules began to appear, which were but few in number, and roſe very large: The child was ſoon well and continued ſo for about a month; when being carried out and kept a long while in the cold, he fell into a feveriſh diſorder, accompanied with a cough, and was ill for four or five days: After that time it went off, and ever after he was in very good health.

A girl about two years of age, in a family where they had formerly buried three children ſucceſſively of the ſmall-pox, was inoculated: The fever came on about the ſeventh day, and ſhe continued very ill till the tenth, on which day about noon ſhe had a ſtrong convulſive fit. In the evening the ſmall-pox appear'd; and tho' ſhe had more in number than uſual, yet ſhe grew well as ſoon as they were fully come out, and continued ſo ever after.

In a family, where they had four children, none of whom had had the ſmall-pox; the eldeſt was ſeiz'd in the natural way with the moſt malignant ſort he ever ſaw, attended with the worſt ſymptoms, inſomuch that he died on the fourth day, all full of purple and livid ſpots: The other three the Dr. inoculated the day before the eldeſt died, after having told the parents, that he could not anſwer for the ſucceſs in caſe they had already catch'd the infection, which would be known, if any of them fell ill before the ſeventh day: Accordingly one of them began on the ſecond day, much after the ſame manner with the eldeſt, and the ſmall pox appeared

appeared on the third day, or rather an univerſal redneſs all over the ſkin, interſpers'd with ſeveral livid ſpots: There were none of theſe ſpots near the places of inſition, which began to ſwell a little, as uſual, about the fourth day, and the ſmall pox roſe a little more about thoſe places than elſewhere; but nature was too far oppreſs'd with the violence of the diſtemper, and tho' this girl continued longer than her brother, and was not ſo delirious as he was, yet ſhe died on the ſeventh day. The Dr. did not reckon this child in the number of thoſe who receiv'd the diſtemper by inoculation; for, he thought there was ſufficient reaſon to conclude, that ſhe had taken the infection before. The other two continued well till the eighth day, when they both fell ill together: The ſmall pox came out on the tenth, of a very good ſort, tho' more in number than ſome others had; and they both got very eaſily over the diſtemper, without any indiſpoſition after. It was obſervable, that the elder of theſe children, about the time of the eruption, had ſeveral ſpots that appeared on him of a deep red colour, very much like thoſe of the other two children, which afterwards changed in them to purple; but when the ſmall pox came out, theſe ſpots grew gradually leſs, and at laſt quite diſappear'd. The other child when very young had been ſubject to convulſions for a long time, and it was very much afflicted therewith from the time that the fever came on, till the ſmall pox appear'd.

A married gentlewoman, about 26 years of age, got very well over the diſtemper; but about a week after, ſhe was ſeis'd with a very great coldneſs and ſhivering, which were followed by a burning heat, with a great pain and diſorder in her head, which continued for ſeveral hours. Some time before ſhe had an intermitting fever, of which he took this to be a paroxyſm, and expected its return; but ſhe felt no more of it, and ever after continued in good health.

The younger of her two ſons, who were both inoculated at the ſame time, got over the diſtemper with a great deal of eaſe, the ſmall pox being few, and the ſymptoms very ſlight; but the elder, a boy about five years of age, far'd quite otherways: The ſymptoms before the eruption were more than uſually ſevere, eſpecially the vomiting; the puſtules appear'd at the uſual time, but more numerous than ordinary, and when the eruption was finiſh'd, the fever did not ceaſe, as it had done in every inſtance but this: On the fifth day after he was ſeis'd, the ſwelling of his face began, which was followed

by a pain and ſwelling in his throat and a ſalivation, which continuing till the 11th day, were ſucceded by a ſwelling in his hands and feet, the uſual ſymptoms of the diſtinct ſort, when they are very full; and tho' there appeared ſome little ſigns of malignity; yet with the uſe of bliſters and the milder cordials and alexipharmics, the puſtules roſe very large, and every thing went on very well; ſo that he got over the diſtemper without any danger, but with much more pain and trouble than any of the reſt had endur'd. After the ſmall pox were gone off, there was a hard ſwelling on his ſhoulder, which diſſabled him for ſome time from moving his arm, but by the uſe of ſome common applications, it was entirely diſcuſſed. The only child they had in this family before theſe died of the ſmall pox, of a malignant ſort, and this boy was of an ill habit, and had ſeveral dangerous illneſſes.

One of the thighs of a girl about nine months old after ſhe was well of the ſmall pox, was a little ſwell'd, which was painful to her for ſome time, and made her unwilling to move that part; there were alſo ſome ſmall tumours in the groin; but theſe went off in a few days, there only remaining a hardneſs about the knee, which alſo diſappear'd in a ſhort time, without coming to a ſuppuration, and the child was reſtored to perfect health.

A girl ſix years of age got very eaſily over the diſtemper; but before the ſmall pox were gone, there was a little tumour on the muſcles of the loins, which ripen'd very ſpeedlly, and was open'd and heal'd up in a very ſhort time. Her ſiſter, a young woman about 18, had alſo a ſwelling of the ſame kind in her leg, but it lay ſomewhat deeper, and gave her a great deal of pain for three or four days; afterwards it diſcharged a great quantity of matter, and heal'd without any farther trouble.

All the reſt, excepting thoſe above-mentioned, got over the diſtemper without any manner of trouble or hazard or any ill conſequence afterwards: Whether thoſe ſlight indiſpoſitions, which ſome have been ſubject to afterwards, were owing to the inſition, the Dr. could not determine: But he preſumes, that what they have endur'd in the courſe of the diſtemper, and what has followed after, is not to be put in the balance with what is undergone in the common way, by thoſe who are thought to come off very well; and if this method were more generally practis'd, it is probable ſome means might be found out, to prevent even theſe ſubſequent diſ-

orders, which are no more frequent, nor near ſo bad, as thoſe which follow the natural ſort.

In two inſtances the inoculation had no effect; the reaſon of which in one was, becauſe the child had had the ſmall pox before, as the parents believ'd; but the diſtemper had been ſo favourable, as to leave it doubtful. In the other the matter was taken, when the puſtules were wither'd and almoſt gone, and that little moiſture which they contain'd, the Dr. ſuppoſes had loſt its virtue; the boy, on whom it was made uſe of, was no ways affected; the places of inciſion did not at all inflame or ſwell, as uſual, nor did any puſtules appear; but about a month after, he was ſiez'd with the diſtemper in the ordinary way, and did very well.

Some of thoſe who had been inoculated, and grown up, have afterwards attended others in the ſmall pox; and it has often happened, that in familes where ſome children have been inoculated, others have been afterwards ſeis'd in the natural way, and they have lain together in the ſame bed all the time; but it has not hitherto been found, that ever any had the ſame diſtemper twice; neither is there any reaſon to ſuppoſe it poſſible, there being no obſervable difference between the natural and artificial ſort; but only that in the latter the puſtules are commonly fewer in number, and all the reſt of the ſymptoms in the ſame proportion more favourable: There is one obſervation the Dr. has made, tho' he would not lay any great ſtreſs upon it; and that is, that in families where any have been inoculated, thoſe who have been afterwards ſeis'd, never had an ill ſort of ſmall-pox, but always recover'd very well.

A farther Account of inoculating the Small-pox; *by the Same.* Phil. Tranſ. N° 370. p. 49.

THE hazard in this method is very inconſiderable, and in proportion to that in the ordinary way by accidental contagion, ſo ſmall, that it ought not to deter any body from making uſe of it.

The Dr. in order to ſatisfy himſelf what proportion the number of thoſe that die of the ſmall-pox might bear to the whole number, ſeis'd with the diſtemper in the natural way, made ſome enquiry, as follows.

In *Halifax*, ſince the beginning of the winter 1721, 276 had the ſmall-pox, out of which 43 died. In *Rochdale* a ſmall neighbouring market town, 177 had the diſtemper, of which

38 died. In *Leeds* 792 had the ſmall-pox, of which 189 died. It is to be noted, that at *Halifax* the ſmall-pox were this ſeaſon more favourable than uſual; and in *Leeds* they were more mortal than uſual: But at a medium in theſe three towns, there died 22 nearly out of every hundred, which is above $\frac{1}{5}$ part of all that were infected in the natural way.

In theſe accounts the Dr. confines himſelf to the limits of the towns; the numbers, that had the ſmall-pox in the country round about, were vaſtly greater; but the proportion of ſuch as died is much the ſame. He made enquiry in ſeverall country villages about *Halifax*; in ſome he found the proportion to be greater, in others leſs, but in the main it was nearly the ſame.

Obſervations of the Variation on board the Royal African Pacquet, *in the Year* 1721; *by Capt.* Cornwall. Phil. Tranſ. N° 371. p. 55.

N. B. The meridional diſtance is reckoned from St. *Jago*.

Month and Year.	Latitudes.			Meridional Diſtance.			Longitud.			Variation.		
Auguſt 24*th* 1721	9°	8′	Sou.	9°	23′	W	9°	25′	W	2°	13′	E
Ditto 26	11	12	S	10	46	W	10	50	W	4	30	E
Ditto 27	11	34	S	11	28	W	11	41	W	4	29	E
Ditto 28	12	32	S	11	31	W	11	43	W	4	27	E
Ditto 31	15	46	S	10	53	W	11	6	W	6	10	E
Septemb. 2*d.*	16	26	S	8	25	W	8	30	W	7	16	E
Ditto 5th	18	45	S	9	31	W	9	39	W	6	17	E
Ditto 6th	19	47	S	9	10	W	10	0	W	8	6	E
Ditto 17	28	43	S	1	7	W	1	9	E	5	53	E
Ditto 22	31	33	S	3	41	E	3	56	E	4	10	E
Ditto 27	33	30	S	11	29	E	12	57	E	0	11	W
Ditto 30	32	40	S	19	6	E	12	1	E	3	0	W
October 1*ſt*,	32	53	S	21	18	E	24	59	E	5	41	W
Ditto 3	32	30	S	25	33	E	30	0	E	7	47	W
Ditto 5	32	28	S	30	37	E	35	52	E	8	44	W
Ditto 6	31	22	S	31	40	E	37	7	E	10	57	W
Ditto 7	31	11	S	32	4	E	37	47	E	11	20	W

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Obſervations on the coaſt of *Africa*.

Month and Year	Latitudes.	Meridional Diſtance.	Longitud.	Variation.
Octob. 13*th*, 1721.	26° 17′ S	35° 35′ E	41° 41′ E	14° 30′ W
Ditto 19	19 41 S			12 22 W
Ditto 21	17 4 S			14 29 W
Ditto 25	13 56 S			14 48 W
Novem. 4*th*,	10 57 S			13 11 W
Ditto 7	8 19 S			15 14 W
Ditto 29	5 0 S	in *Cabenda-Bay*		14 33 W
From *Cabenda* to *London*, Meridional Diſtance from thence				
Decem. 9*th*,	3 25 S	11 38 W	11 43 W	11 32 W
Ditto 14	3 30 S	21 18 W	21 24 W	
Ditto 20	0 30 S	30 41 W	30 46 W	1 5 W
Jan. 1*ſt* 172½	10 50 N	39 8 W	39 16 W	1 1 E
Ditto 6	17 15 N	43 21 W	43 29 W	1 41 E

Remarks on an Experiment, whereby it has been attempted to ſhew the falſity of the common Opinion, in relation to the Force of Bodies in Motion; by Dr. Pemberton. Phil. Tranſ. N° 371. p. 57.

DR. *Pemberton*, peruſing the learned *Polenus*'s tract *de Caſtellis*, found ſeveral curious experiments therein; among which he reckons that of letting globes of equal magnitude, but of different weights, fall upon a yielding ſubſtance, as tallow, wax, clay, &c. from heights reciprocally proportional to the weights of the globes. This experiment, being brought on purpoſe to overturn one of the firſt principles, eſtabliſhed in natural philoſophy, the Dr. can by no means admit of the deduction drawn from it, *viz.* that becauſe the globes make in this experiment equal impreſſions in the yielding ſubſtance, therefore, they ſtrike upon it with equal force; whereby it is attempted to prove the aſſertion of M. *Leibnitz*, that the force of the ſame body in deſcending is proportional to the height from whence it falls; or in all motion, proportional to the ſquare of the velocity, and not proportional to the velocity itſelf, as is commonly taught. On the contrary, the Dr. thinks, that this very experiment proves the great unreaſonableneſs of M. *Leibnitz*'s notion.

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It is ſurpriſing, that ſo careful a writer, as *Polenus* appears to be, from the accuracy with which he delivers his experiments, ſhould not rather ſuſpect his reaſoning in an intricate caſe, than thus contradict a principle in philoſophy, that has been directly proved by a multitude of experiments, in particular, by thoſe Sir *Iſaac Newton* recommends for that purpoſe *Princip. Philoſ. Nat. p.* 19. and that is, moreover, abundantly eſtabliſhed, by its exact agreement with all obſervations; as being the principle upon which all appearances, hitherto obſerved in the motion of bodies, are accounted for by juſt and undeniable arguments; and we ſhall find upon enquiry, that the preſent caſe comes alſo under the ſame rule.

As the uſe of experiments in *Natural Philoſophy* is to diſcover the cauſes of things, by exhibiting more ſimple effects of thoſe cauſes, than occur in the ordinary courſe of nature: So for this end it is neceſſary, that our argumentation upon experiments be perfectly juſt, otherways they may lead us into errors. The firſt thing, neceſſary for making right deductions from an experiment, is to determine the proper uſe thereof; which, in the experiment before us, the Dr. thinks is not rightly underſtood. Certainly this experiment of *Polenus* is much more adapted to inform us of the law, by which theſe yielding ſubſtances reſiſt the motion of bodies ſtriking upon them, than to ſhew the forces with which bodies ſtrike; for, whatever thoſe forces be, the effects muſt be very different, according to the difference there may be in the rule, obſerved by ſuch reſiſtance.

Now this experiment ſhews, that if two globes in motion bear againſt equal portions of the yielding ſubſtance, the oppoſition, that ſubſtance makes to the motion of the globes, will be the ſame in both, however different the velocities be, with which they move. This the Dr. demonſtrates as follows.

Let A and B be two globes of equal magnitude but of different weights, which are equally immerſed into a yielding ſubſtance: Suppoſe the velocities with which they move in their preſent ſituation, to be reciprocally in the ſubduplicate ratio of the weights of the globes; that is, let the ratio of the weight of the globe A to the weight of the globe B, be duplicate of the ratio of the velocity of the globe B, to the velocity of the globe A: Since therefore the ratio of the quantity of motion in the globe A, or of the force with which it moves, to the quantity of motion in the globe B, or to the force with

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which that globe moves, is compounded of the ratio of the weight of the globe A to the weight of the globe B and of the ratio of the velocity of the globe A to the velocity of the other globe B; the force, with which the globe A moves, is to the force with which the globe B moves, as the velocity of this globe B to the velocity of the other globe A. But if the ſame oppoſition be made to the motion of the globes, when they bear upon equal portions of the yielding ſubſtance, the effect of that oppoſition, while the globes enter farther into the ſubſtance by equal ſpaces, will be proportional to the time, in which the globes are moving thoſe ſpaces, or in which the oppoſition is made, if we conſider thoſe ſpaces while naſcent, or in their firſt origin; the effect therefore of this oppoſition will be reciprocally proportional to the velocity of each globe; namely, the momentaneous loſs of force in the globe A will be to the momentaneous loſs of force in the globe B, as the velocity of the globe B to the velocity of the globe A; and the whole force of the globe A has been found to bear the ſame ratio to the whole force of the globe B; conſequently theſe globes, while they penetrate equal ſpaces into the ſubſtance, loſe parts of their force, that bear the ſame proportion to the whole; and therefore, if their velocities be at any time reciprocally in the ſubduplicate ratio of their weights; ſo that the forces or degrees of motion, with which they move, be reciprocally proportional to their velocities, the forces with which they preſs, into the yielding ſubſtance, at equal indentures, made in the ſubſtance, will continue in the ſame proportion; and therefore, upon the theory of reſiſtance here ſuppoſed, when the whole force and motion of both theſe globes is entirely loſt, they will be plunged into the ſubſtance, at equal depths.

Now whereas in the experiment of *Polenus*, the globes falling from heights reciprocally proportional to their weights, ſtrike upon the yielding ſubſtance with velocities reciprocally in the ſubduplicate ratio of their weights, and the effect is in all caſes found to be what is here deduced from this theory of reſiſtance, it is a ſufficient confirmation of the truth of this theory.

Only here the Dr. ſuppoſes the globes to be ſtopped by the whole reſiſtance of the ſubſtance they move againſt; tho' ſtrictly ſpeaking, they are ſtopped only by the exceſs of that reſiſtance above the action of gravity upon them: But he neglects the conſideration of the action of graviry, that being but

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ſmall in proportion to the reſiſtance, as will appear from the globes being much more ſpeedily ſtopped by this reſiſtance, than they would be by the action of gravity, if its force were applied upwards; for, by that force alone, the globes would not be ſtopped, till they had gone over ſpaces, equal to the heights above the reſiſting ſubſtance, from whence they fell; which heights bear a great proportion to the depths into which the globes in this experiment are immerſed in the yielding ſubſtance, as he found upon trial.

Thus, the Dr. takes it, the difficulties attending this experiment may be removed: But as M. *Leibnitz*'s opinion is deduced from it by means of this axiom, that effects are proportional to their cauſes; ſo that here the effects being thought the ſame, the cauſes are concluded to be ſo likewiſe; the Dr. mentions an experiment, where this axiom may be more juſtly applied, than it can be in the preſent caſe, from which experiment the received opinion may be proved: This experiment is mentioned by *Polenus* from *Merſennus*, tho' tried by him ſomewhat rudely; but has been often ſince made in the following manner; a weight is ſuſpended at one end of the beam of a balance, and from a proper height is let fall upon the other end of the beam another weight, which by ſtriking thereon ſhall raiſe the end, to which the weight is ſuſpended, to ſuch a height, as is juſt ſufficient to ſet free a certain ſpring; if then a different weight be hung in the room of the former, the height from which the falling weight muſt deſcend, in order to raiſe the extremity of the balance, to which this other weight is ſuſpended, to the ſame height as before, that is, high enough to ſet the aforeſaid ſpring at liberty, is found to be ſuch, that the velocity, with which the falling weight ſtrikes upon the balance, in this latter caſe, will be to its former velocity, as the latter weight to the former, vide *Graveſande's Phyſic. Elem. Tom.* I. p. 39. excepting only, that agreeably to what *Merſennus* himſelf remarked, when the weight deſcends from conſiderable heights, an altitude, ſomewhat greater than this rule implies, is required to raiſe the other weight, as much as is deſired: But whether the bending of the arm of the balance, when acted upon with a conſiderable force, or whether any increaſe of friction in this caſe, occaſion the irregularity here mentioned, we need not ſtrictly enquire; for, this irregularity is ſtill leſs reconcileable with the new opinion than the regular effects of the experiment. Hence, therefore, we may ſee, that the very method of reaſoning, which being applied

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erroneouſly, is ſuppoſed to prove M. *Leibnitz*'s ſentiment concerning the force of bodies in motion, will, when juſtly uſed, confirm the other opinion in relation to that matter.

That the very experiment of *Polenus* is not only reconcileable to the common doctrine of motion, as the Dr. has now demonſtrated; but even that it does itſelf make manifeſt the great unreaſonableneſs, if not the abſolute abſurdity of M. *Leibnitz*'s opinion, he proves, as follows.

If two globes as A and B, of equal magnitude, but different weights, ſtriking on a yielding ſubſtance, with equal force, in every caſe loſe all their motion at equal depths; it is neceſſary, that at all times, during their motion, they loſe equal degrees of force, when they bear upon equal portions of the ſubſtance, in entering equal ſpaces thereof. This will be eaſily ſeen from what has been ſaid before. Now, whereas M. *Leibnitz* ſuppoſes the power of gravity to give to the ſame falling body degrees of force, proportional to the height from whence it falls, according to his opinion, by the power of gravity, equal degrees of force are added in the deſcent of the ſame body thro' equal ſpaces; and in different bodies, deſcending thro' equal ſpaces, the degrees of force added will be as the quantity of matter, or as the weight of each body. Therefore while the globes A and B penetrate equal naſcent ſpaces into the yielding ſubſtance, by the action of gravity, were not that action overcome by the reſiſtance of that ſubſtance, additional degrees of force would be communicated in ſuch proportion, that the force added to the globe A, would be to the force added to the globe B, as the weight of the globe A, to the weight of the globe B, or in the duplicate ratio of the velocity of the globe B, to the velocity of the globe A. But ſince the globes loſe the ſame degrees of force in entering equal naſcent ſpaces into the yielding ſubſtance, the effect of the oppoſition made by this ſubſtance to the motion of the globes, during the time of their paſſing thro' ſuch naſcent ſpaces, will be both the taking from them that ſame degree of force, and moreover the additional force, which would otherways have been given them by their own gravity: But farther, the oppoſition made to the motion of the globe A to the oppoſition made to the motion of the globe B, will be in the ratio compounded of the ratio of the effect of the oppoſition, the ſubſtance makes to the motion of the globe A, to the effect of the oppoſition, the ſubſtance makes to the motion of the globe B, and of the ratio of the time, in which the oppoſition is made againſt the latter globe, to the time in which it is made againſt the former; which latter ratio

is the ſame with the ratio of the velocity of the globe A to the velocity of the globe B. But ſince it is ſhewn, that the effect of the oppoſition made by the yielding ſubſtance to theſe globes is twofold; and that one part of the effect of the oppoſition made to the motion of the globe A is equal to one part of the effect of the oppoſition, made to the motion of the globe B; and that another part of the effect of the oppoſition made to the motion of the globe A, to another part of the effect of the oppoſition made to the motion of the globe B, is in the duplicate ratio of the velocity of the globe B, to the velocity of the globe A; one part of the oppoſition itſelf made to the motion of the globe A will be to one part of the oppoſition againſt the motion of the globe B, as the velocity of the globe A, to the velocity of the globe B; and another part of the oppoſition to the motion of the globe A, to another part of the oppoſition to the motion of the globe B, will be as the velocity of the globe B, to the velocity of the globe A: So that when the globes bear upon equal portions of the yielding ſubſtance, the oppoſition to their motion will be in part as the velocity of the globes; and in part reciprocally as their velocity. Hence becauſe the reſiſting ſubſtance is of an uniform texture, the oppoſition to the motion of either of the globes in its preſent ſituation, and when moving with its preſent velocity, will be to the oppoſition it would meet with in the ſame ſituation, if it moved with any other velocity, partly as the preſent velocity to that other velocity, and partly as that other velocity to the preſent. But by that part of the oppoſition made againſt the motion of the globe, which is directly as the velocity, the globe can never be wholly ſtopped; for, upon the ſtopping of the globe, that part of the oppoſition to its motion will likewiſe totally ceaſe; and conſequently, the weight of the globe will carry it farther down, unleſs the other part of the oppoſition againſt its motion prevent it: But, neither can this latter part of the oppoſition made to its motion be ever great enough to ſtop the globe; for, the degree of this oppoſition being reciprocally as the velocity of the globe, when the motion of the globe is entirely taken away, it will become infinitely greater than at any time, while the globe is in motion; ſo that when the globe ſhould be ſtopped by this part of the oppoſition made to its motion, the oppoſition to the motion of the globe will become infinitely great; inſomuch, that no degree of force whatever could be able to impel the globe farther into the ſubſtance; but this can never happen: Beſides, it is not neceſſary to apply any ſuch refined argument againſt this part of

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the reſiſtance; it would be alone ſufficient to conſider, how unreaſonable a ſuppoſition it is, that a reſiſtance ſhould increaſe, when the velocity of the reſiſted body decreaſes.

Thus may this experiment be made uſe of to invalidate that very opinion it is brought to ſupport: But another uſe may likewiſe be made thereof; for it will ſerve to illuſtrate what the great Sir *Iſaac Newton* has more than once hinted, *viz.* that the reſiſtance of fluids, ariſing from the tenacity of their parts, decreaſes in a leſs proportion than the velocity of the reſiſted body does. vide *Philoſ. Nat. Prin. Math. Prop.* 52. *lib.* 2. in *Schol. Optics Qu.* 28. *p.* 339, 340. for, as this reſiſtance bears a great analogy to the reſiſtance of the yielding ſubſtances here treated of; ſo it is found, that the reſiſtance of theſe ſubſtances does not much depend upon the velocity of the body, againſt which the reſiſtance is applied.

And thus we may ſee, how all experience conſpires in confirming and ſetting forth that ſtupendious force of reaſoning, which has enabled our great philoſopher moſt ſurpriſingly to ſearch out and diſtinguiſh the ſprings of natural operations; a work infinitely more difficult to accompliſh than even the great improvements he has made in pure mathematics, which were previouſly neceſſary in order to his ſucceeding in his reſearches into the knowledge of nature; for, in this laſt purſuit he has given proof, not only of a more unbounded invention, than is required in the ſubtileſt geometrical ſpeculations; but has alſo there diſcover'd the greateſt diſcernment and moſt conſummate judgment: Since in his philoſophical writings, he has never been once impos'd on by an hypotheſis, nor by any other of the various fallacies, which my Lord *Bacon* in his *Novum Organon* has reckoned up as the cauſes, that had hinder'd the improvement of the true philoſophy.

Another very curious and weighty argument to confirm Sir *Iſaac Newton*'s ſentiments in relation to the reſiſtance of fluids, is as follows.

Suppoſe pieces of fine ſilk or the like thin ſubſtances, extended in parallel planes and fixt at ſmall diſtances from each other; then ſuppoſe a globe to ſtrike perpendicularly againſt the middle of the outermoſt of the ſilks, and by breaking thro' them to loſe part of its motion: If the pieces of ſilk be of equal ſtrength, the ſame degree of force will be requiſite to break each of them; but the time in which each piece of ſilk reſiſts, will be ſo much ſhorter, as the globe is ſwifter; and the loſs of motion in the globe, conſequent upon its breaking thro' each ſilk, and ſurmounting the reſiſtance

thereof, will be proportional to the time, in which the silk oppoſes itſelf to the motion of the globe: Inſomuch that the globe by the reſiſtance of any one piece of ſilk will loſe ſo much leſs of its motion, as it is ſwifter. But on the other hand, by how much ſwifter the globe moves, ſo many more of the ſilks it will break thro' in a given time: Whence the number of the ſilks which oppoſe themſelves to the motion of the globe in a given time, being reciprocally proportional to the effect of each ſilk upon the globe, the reſiſtance made to the globe by theſe ſilks, or the loſs of motion the globe undergoes by them in a given time, will be always the ſame.

Now if the tenacity of the parts of fluids obſerve the ſame rule, as the coheſion of the parts of theſe ſilks; namely, that a certain degree of force is required to ſeparate and diſunite the adhering particles, the reſiſtance ariſing from the tenacity of fluids muſt obſerve the ſame rule, as the reſiſtance of the ſilks; and therefore in a given time the loſs of motion a body undergoes in a fluid by the tenacity of its parts, will in all degrees of velocity be the ſame; or in a word, that part of the reſiſtance of fluids, which ariſe from the coheſion of their parts, will be uniform.

An Account of the Falls of the River Niagara, *taken at* Albany Oct. 10. 1721. *from M.* Boraſſaw, *a Native of* Canada; *by Mr.* Dudley. Phil. Tranſ. N° 371. p. 69.

THE falls of *Niagara* are a mighty ledge or precipice of ſolid rock, that lies a-croſs the whole breadth of the river (a little before it empties itſelf into or forms the lake *Ontario*) and very ſteep.

M. *Boraſſaw* ſays, that in ſpring, 1722, the Governor of *Canada* order'd his own ſon with three other officers to ſurvey *Niagara* and take the exact height of the cataract, which they accordingly did with a ſtone of half a hundred weight, and a large cod-line, and found it upon a perpendicular no more than *vingt et ſix bras.*

This account differs from that F. *Hennepin* has given the world of that cataract; for, he makes it 100 fathoms; and our modern maps from him, (as Mr. *Dudley* ſuppoſes) mark it at 600 foot; but he believes *Hennepin* never meaſured it, and there is no gueſſing at ſuch things.

Upon Mr. *Dudley*'s objecting *Hennepin*'s account of thoſe falls to M. *Boraſſaw*, he replied; that accordingly every body had depended upon it as right till that late ſurvey. Upon farther converſation he acknowledged, that below the cataract

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for a conſiderable way, there were a great many ſmall ledges or ſtairs croſs the river, that lower'd it ſtill more and more, till you come to a level: So that if all the deſcents be put together, he does not know but the difference of the water above the falls, and the level below may come up to F. *Hennepin*'s account; but the cataract, properly ſo call'd, is, upon the perpendicular, no more than 26 fathoms or 156 foot, which is a prodigious thing, and what the world cannot parallel, conſidering the greatneſs of the river; for, it is near $\frac{1}{4}$ of an *Engliſh* mile broad, and very deep water.

It has been alledged, that the cataract makes ſuch a prodigious noiſe, that people cannot hear each other ſpeak, at ſome miles diſtance; but M. *Boraſſaw* affirms, that you may converſe together cloſe by it.

It has been poſitively aſſerted, that the ſhoot of the river, when it comes to the precipice, was with ſuch a mighty force, that men and horſe might go under the body of the river without being wet; this M. *Boraſſaw* utterly denies, and ſays the water falls in a manner right down.

What M. *Boraſſaw* farther obſerv'd was, that the miſt or ſhower *(la brume)* which the falls occaſion, is ſo extraordinary, as to be ſeen at five leagues diſtance, and riſe as high as the common clouds: In this *brume* or cloud, when the ſun ſhines, there is always a glorious rainbow.

The river itſelf, which is there call'd the *Niagra*, is much narrower at the falls than either above or below; and from below there is no coming nearer the falls by water than about ſix *Engliſh* miles, the torrent is ſo rapid and withal ſuch terrible whirl-pools.

M. *Boraſſaw* confirms F. *Hennepin*'s and M. *Kellug*'s account of the large trouts of thoſe lakes, and he affirm'd there was one taken, that weigh'd 86 pounds, which Mr. *Dudley* is the more apt to believe upon the general rule, *viz.* that fiſh are according to the waters: To confirm which, a miniſter affirm'd, that he ſaw a pike taken in *Canada* river and carried upon a pole between two men, that meaſur'd five foot 10 inches in length, and proportionably large.

Mr. *Dudley* ſaw a cataract, three leagues above *Albana* in the province of *New York*, upon *Schenectada* river, call'd the *Cohoes*, not above 40 or 50 foot perpendicular: From theſe falls there alſo ariſes a miſty cloud, which deſcends like ſmall rain, and when the ſun ſhines, yields a beautiful ſmall rainbow, that moves as you move, according to the angle of viſion. The river at the *Cohoes* is from 40 to 50 rods broad; but then

it is very ſhallow water; and he was told, that in a dry ſeaſon, the whole river runs in a channel of no more than 15 foot wide.

In his journey to *Albany*, 20 miles to the eaſtward of *Hudſon's* river, near the middle of a long riſing hill, Mr. *Dudley* met with a briſk noiſy brook, ſufficient to turn a water-mill, and having obſerv'd nothing of it at the beginning of the hill, he turn'd about and followed the courſe of the brook, till at length he found it come to an end, being abſorbed and ſinking into the ground; either running thro' ſubterraneous paſſages, or ſoak'd up by the ſand; and tho' it be common in other parts of the world for brooks and even rivers to be thus loſt; yet this is the firſt of the ſort, he ever heard of or met with in this country.

An Account of the Muſcular Fibres in ſeveral Animals, and the Magnetic Quality acquir'd by Iron, upon ſtanding for a long Time in the ſame Poſition; by M. Leewenhoeck. Phil. Tranſ. N° 371. p. 72.

M. *Leewenhoeck*, upon viewing a portion of the fleſh of a fat ox, as likewiſe the muſcular fibres of a cod-fiſh and of a pearch, that were cut tranſverſely, could very diſtinctly obſerve the great number of ſmall veſſels, that ran along the length of each fibre; and he has ſeen the ſame in the muſcular fibres, taken from the hinder leg of a mouſe, and cut thro' tranſverſely.

M. *Leewenhoeck* had ſtanding before a microſcope ſome of the muſcular fibres of a fat ox, together with thoſe of a mouſe lying by them, in order to have as many eye-witneſſes as poſſible of their being of the ſame ſize in thoſe two animals; and he uſed the ſame method in ſuch other of his obſervations, as were likely to appear incredible, letting the object lie before the microſcope day after day, and ſometimes for whole years together, till it was eaten up by inſects.

As to the ſmall fibrils, mentioned in a former *Tranſaction*, that help to ſuſpend the *teſtes*, it is to be obſerv'd, that each of them conſiſts of exceeding ſmall veſſels, which run parallel to its length.

M. *Leewenhoeck* had likewiſe before his microſcope, a ſmall portion of an ox's bone, in which might plainly be obſerv'd, the veſſels which proceed from the bone and compoſe what is call'd the *perioſteum*, as likewiſe the openings of theſe veſſels; the reaſon of whoſe appearing ſo plainly is, as he ſuppoſes, becauſe they are fill'd with the medullary oil.

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The iron croſs, which is ſuppos'd to have ſtood upon the ſteeple of the new church at *Delft* about 200 years, having been taken down to be repair'd, and M. *Leewenhoeck* being inform'd by a certain foreign gentleman, that a piece of iron, that has ſtood for a long time in one poſition, would thereby acquire a magnetic quality, he procur'd a bit of that croſs, of about a ſpan long and a quarter of an inch thick, which he applied both to a working needle and the needle of the compaſs, but without any effect upon either.

Some time after, M. *Leewenhoeck* had brought him ſome other pieces, looking like ruſty iron, that had been broke off from the bottom of the ſaid croſs, where it had been faſtened by four croſs pieces, bound down with iron, to an erect piece of timber nine inches ſquare, and cover'd with lead in ſuch a manner as no wet could get to it.

This ſeeming ruſty iron would take up ſeveral needles hanging by each other; and it appear'd to have a ſtronger magnetic virtue than two loadſtones, he had then in his cuſtody, and ſo hard that no file would touch it.

M. *Leewenhoeck* gave one of the largeſt pieces to a knife-grinder, to grind, who was a long time about it, and complain'd that it was harder than ſteel.

The Method of bending Planks *in his Majeſty's Yards at* Deptford, *&c. by a Sand-heat, invented by Capt.* Cumberland; *by Mr.* Cay. Phil. Tranſ. N° 371. p. 75.

THE place, where the planks lie to be ſoftened in the ſtove, is between two brick walls, of ſuch a length, height and diſtance from each other, as is ſufficient to admit the largeſt or to hold a good number of the ſmaller ſort; the bottom is of thick iron plates, ſupported by ſtrong bars; under the middle of which are two fire-places, whoſe flews carry the flame towards the ends.

The planks are laid in ſand; the loweſt about ſix or eight inches above the iron plates; they are well cover'd with the ſand and boards laid over all to keep in the heat; the ſand is moiſtened with warm water (for which purpoſe they have a cauldron adjoining to the ſtove) and if the timber be large and intended to be very much bent, ſo that it muſt lie long in the ſtove, they water the ſand again, once in eight or ten hours. When it is judged to be ſoft enough, the ſand is remov'd; and the workmen carry away their reſpective planks to the ſeveral places, where they are to be us'd, and having

firſt nail'd a thin board upon the outſide to preſerve the plank from bruiſes, they fix one part in its proper place, and bring to the others, by any power they can moſt conveniently apply. This work ſeems to be performed with very great eaſe; notwithſtanding ſome were ſo knotty, that the builders aſſur'd they could not be brought to that curvature by the former methods. The planks put in between others, very exactly fitted the ſpaces they had been cut for; and the workmen told, they had made no allowance for the ſwelling or ſhrinking of the wood.

This method excells that of burning the planks over an open fire, in ſeveral reſpects; particularly, that no part of the wood is deſtroy'd, but remains of the ſame dimenſions, at leaſt very nearly; a plank of the breadth of 16 inches being ſaid not to alter above $\frac{1}{20}$ part of an inch; the edges of the plank are preſerv'd; and conſequently, the work muſt be much firmer, and the calking laſt longer; the extraordinary ſoftneſs of the wood, while it is warm, makes it eaſily bend to any figure, neceſſary in ſhip-building, which it holds very well if they have occaſion to take it off again after it is cold: Whereas the plank, bent by burning, would ſtart, when looſened; and could only be fixt to the timbers by ſuch a force, as could overcome the reſiſtance, occaſioned by the ſpring of the plank: It likewiſe adapts itſelf very readily to the ſurface of the timbers, if they happen to be uneven.

The gun-deck clamps in a ſhip of the ſecond rate; which are very large planks, and wont to be cut into ſhape, were bent and twiſted in ſo peculiar a manner, as could not be effected by any other method: The whole operation is perform'd with much leſs trouble to the carpenters, as well as at leſs expence; and they hope the wood will be more durable; as is evident from the deep tincture the ſand receives, by a conſiderable quantity of ſap coming out of the oak, while it is in the ſtove; and a large plank was obſerv'd by the workmen or officers belonging to the yard, to weigh ſome pounds leſs, when it was taken out.

A plank five inches thick requires five or ſix hours to make it fit for bending; and the time requiſite for others ſeems to be in a duplicate ratio of their thickneſs.

Fig. 1. Plate VI. repreſents a plank in the buttocks of a ſecond rate, whoſe length from A to C is three foot, and thickneſs A F four inches and $\frac{1}{4}$; the end C of this plank was bent 12 or 13 inches from the ſtreight line A B.

Figs

PLATE V.

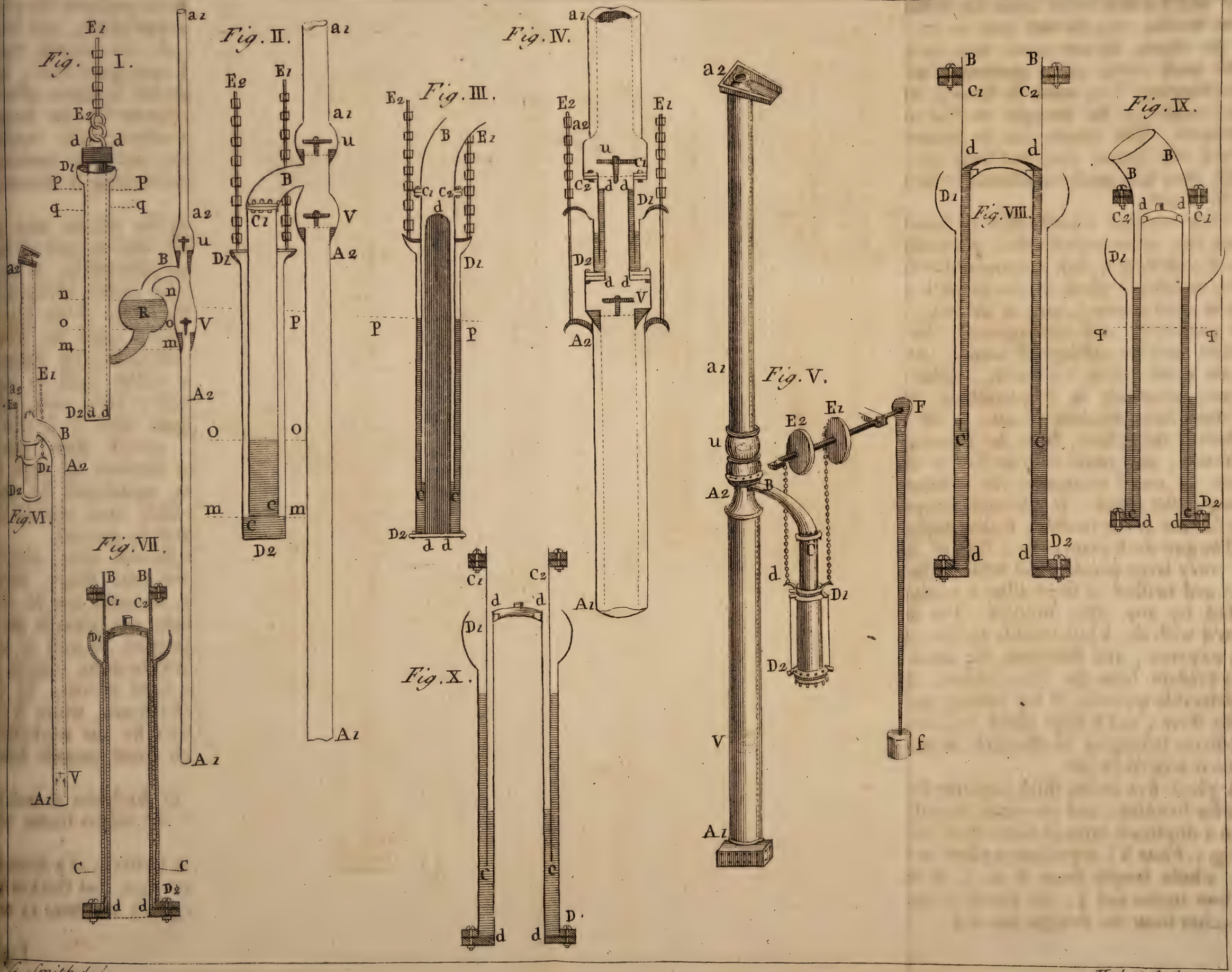


Fig. I.
Fig. II.
Fig. III.
Fig. IV.
Fig. V.
Fig. VI.
Fig. VII.
Fig. VIII.
Fig. IX.
Fig. X.
E1
E2
a1
a2
d
D1
D2
P
q
B
u
V
A2
A1
n
o
m
R
C1
C2
c
C
F
f
G. Smith del.
Hulett Sculp.

Fig. 2 and 3. repreſent ſections of the ſtove; A A the fire-places; B B the aſh-holes; C C the flews under the iron bottom; D D the two chimneys; E the place for the planks and ſand; F F the two brick-walls; G G two inclined planes for the men to ſtand on, &c. when they put in or take out planks, or water the ſand; *h h* the bottom of the ſtove, made of iron; *i i* the grates to lay the fewel on.

An Account of two Caſes of Wounds *in the* Stomach; *by Mr.* James Field. Phil. Tranſ. N° 371. p. 78.

A Luſty young *Negro* in *Antegoa*, returning home about noon, went into his houſe, where ſeeing ſome ripe plantains, he eat of them heartily; his father-in-law, about 60 years of age, coming to the ſame houſe ſoon after, and finding the young fellow had taken his plantains, gave him a moſt deſperate wound with his knife in the upper region of the belly, a vaſt gaſh being made in the ſtomach; inſomuch that the plantains he had eaten, burſt thro' the wound, which was made ſtreight up and down.

The old man immediately fled for it; the young fellow's companions upon hearing what was done purſued him with bills in their hands: The old man ſeeing them follow him in this manner and get ground of him, and ſuſpecting their deſign was to kill him, pulled out the ſame knife, with which he had ſtabb'd the other, and gave himſelf as deſperate a wound, as he had given him, and in the upper region of the belly, his ſtomach being likewiſe ſeen, only with this difference, that this laſt wound was tranſverſe or from left to right, the firſt directly up and down: The old man was carried home and laid in the ſame houſe, where the other lay.

This accident happened about noon, and the ſurgeon did not come to dreſs them till between four and five; he ſtitched up both their ſtomachs and bellies, only leaving in each a ſmall hole for ſuppuration: A fever ſeiſed each of the patients; the old man was in greateſt danger; the fever held them about a fortnight; the wounds were brought to a good digeſtion, and in a month's time or thereabouts the young fellow went abroad; but the old man lay ſomewhat longer: They were both perfectly cured, and very well ever after.

An Account of an Impoſthumation *in the* Stomach; *by Mr.* Atkinſon. Phil. Tranſ. N° 371. p. 80.

MR. *Atkinſon* had a patient, who had a large tumour on the upper part of her belly; it was hard and painful; but did not alter the natural colour of the ſkin and was three months in coming: He applied a warm gum-plaiſter, which in about a fortnight's time brought it to a ſuppuration; he then applied a cauſtic about the bigneſs of a ſhilling; when the eſcar fell off he obſerved a ſolid kind of ſubſtance appear in the orifice; he laid hold of it with his *forceps* and pulled it gently towards him; upon which there thruſt forcibly out a quantity of it that near filled his hand, and ſo he dreſſed it. Next dreſſing the ſame ſubſtance appeared again, which on her ſtraining, forced out twice as much as before: Mr. *Atkinſon* concluded it was the *omentum*, in which opinion he was confirmed by ſome other ſurgeons, to whom he ſhewed it: He ſtill doubted, whether the ſtomach was concerned in this caſe or not, till the next removal of the dreſſings; at which there ſpurted out upwards of ½ a pint of ale in a full ſtream, which was part of a pint the patient had drank a little before: Mr. *Atkinſon* now concluded the caſe mortal; however he ordered the patient to keep her bed, to lie conſtantly upon her back and feed on things of eaſy digeſtion. The greateſt part of what ſhe eat or drank came thro' the ulcer for 8 or 10 days; ſo that he had no hopes of ever curing it, yet contrary to his expectation in about ſix weeks ſhe was perfectly cured.

An Account of the Quantity of Reſin *in the* Cortex Eleutheriæ; *by Mr.* John Brown. Phil. Tranſ. N° 371. p. 81.

DR. *Douglaſs* having given an account (from the hiſtory of the *Royal Academy* at *Paris*) of the *cortex Eleutheriæ*; and among other things having ſaid of it, that M. *Boulduc* had, by means of ſpirits of wine, gotten from an ounce of the bark five drachms of reſinous extract, there remaining three drachms of *fæces*, and that Gentleman's account of ſome of the properties of this bark being founded on the quantity of reſin, ſuppoſed to be contained therein; Mr. *Brown* propoſed to Dr. *Douglaſs* and ſome other Gentlemen of the *Royal Society* (who agreed with him in believing, that ſcarcely any part of any plant whatſoever would yield that quantity of reſinous extract) to try the experiment, which was performed in the following manner.

He

He took two ounces of pick'd bark and digeſted it in rectified ſpirits of wine, which was often decanted and freſh ſpirits put on, till the bark would yield no more tincture; the impregnated ſpirits being evaporated by a very gentle fire; there was left two drachms of reſinous extract; the remains of this dried, weigh'd one ounce, two drachms and $\frac{1}{2}$; the loſs this way is three drachms and $\frac{1}{2}$.

He boiled theſe remains in ſeveral waters, till they would no longer tinge the water, which being evaporated, yielded a drachm and a half of extract; the remains of this dried, weighed one ounce, and half a drachm; the loſs by this method is half a drachm.

He took two ounces more of pick'd bark and boiled it in ſeveral waters, till the bark gave no more colour; and then, upon an evaporation of the water, he had two drachms of extract; the remains being dried weigh'd one ounce and ſix drachms; here the loſs was nothing, excepting ſo much as might anſwer in weight to the quantity of the menſtruum left in the extract, which allowance muſt likewiſe be made in the other extracts.

He digeſted the remains in rectified ſpirits of wine, till they no longer tinged the ſpirits; and by a very gentle evaporation, he found remaining one drachm of reſinous extract: What was left, when dried, weighed one ounce, two drachms, and a half; in this the loſs was two drachms and a half.

The difference in the quantity of extract, obtained by theſe two different methods, is but half a drachm; and the medium between them, upon putting together the ſeveral extracts, made with ſpirits of wine and water, is in the whole but three drachms and $\frac{1}{4}$. But the extract made with ſpirits of wine alone is no more than two drachms from two ounces of the *cortex*, inſtead of 10 drachms, which it ſhould have yielded according to M. *Boulduc*.

An Account of the new Method of Cutting for the Stone; *by Mr.* Douglas. Phil. Tranſ. N° 371. p. 83.

EVER ſince Mr. *Douglas* could reaſon upon the cauſes of the tedioufneſs and bad ſucceſs of the common methods of cutting for the ſtone, he concluded they were principally owing to the natural ſtructure and ſituation of the parts concerned.

Therefore, he began to conſider, why that operation might not be performed the highway, ſo frequently mentioned by authors, but never approved of by any, excepting the moſt ſagacious of all ſurgeons, *Fr. Roſſetus*, who, he thinks, has either been very

little read, or very ill underſtood, elſe this operation had not been ſo long a ſecret.

After making ſome experiments on dead bodies, Mr. *Douglass* was convinced the ſtone might be extracted that way with a great deal leſs trouble than in the common way; and he was perſuaded the wound would heal again, by the great number of authentic inſtances of accidental wounds in the ſame place being ſpeedily and firmly cured; and therefore he reſolved to make the experiment on the firſt patient he could meet with, which was in *December* 1719, and then he proceeded in the following manner.

The patient was placed flat on his back on a table, with a pillow under his head; then his wriſts and ankles were faſtened together, with ſtraps; then he ordered one aſſiſtant to his head, another to each of his ſhoulders, two to the *penis*, one of which was to manage the ligature and the other the *præputium*, and one to each knee, to hold them as faſt and firm as poſſible.

The patient and aſſiſtants being thus placed, the operation conſiſts of three parts.

1. In filling the bladder, which is done thus; paſs the catheter, as repreſented by Fig. 4. Plate VI. then draw out the ſtillet Fig. 5. then order the ligature-aſſiſtant to caſt the ligature, which is a ſkein of ſilk, round the *penis*, above the glans; then fix the key Fig. 6. to the head of the catheter Fig. 7, to keep it ſteady, while you ſcrew on the ſyringe Fig. 8. then ſcrew the ſecond part of the ſucking pipe Fig. 9. to the firſt part Fig. 10. then order the *penis*-ligature to be ſtraitened and the *præputium*-aſſiſtant to gather the *præputium* up about the catheter and to hold it as cloſe as poſſible; then order the water, being a little warmer than milk, to be clapp'd under the ſucking pipe; then draw up the water into the ſyringe and thruſt it into the bladder at leiſure, and repeat it till the bladder be ſo full, that you can perceive its tumour thro' the *abdomen* (at which time you may alſo obſerve the *penis* above and the *præputium* below the ligature, very much ſwelled and the patient in a great deal of pain) which is a certain ſign that there is enough injected; then withdraw your ſyringe and catheter together, taking particular care, that your *penis*-aſſiſtants ſtraiten their gripe, leaſt the water ſhould follow the catheter, which would undo all.

2. In making the inciſion, which is done thus; order the *penis*-aſſiſtants to turn the *penis* towards the *anus*, that ſo their hands may be the more out of the way, then take the firſt knife, repreſented by Fig. 11. and cut at leiſure and with a ſteady hand, from near the upper or lower part of the tumour of the bladder, according

PLATE VI.

Fig. XXIII.

Fig. I.

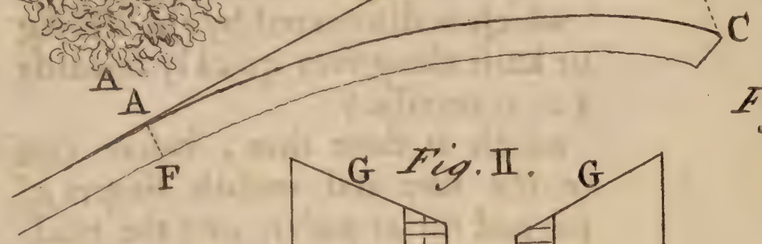


Fig. VI.

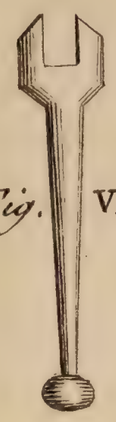
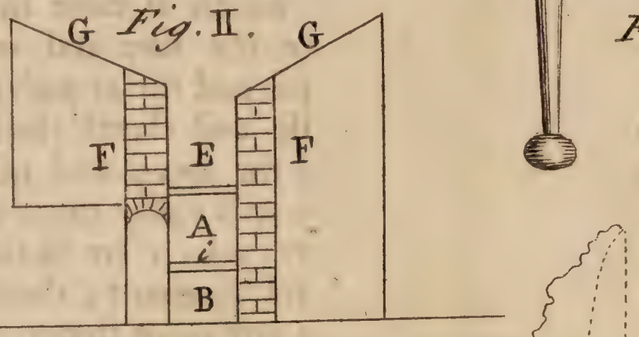


Fig. II.



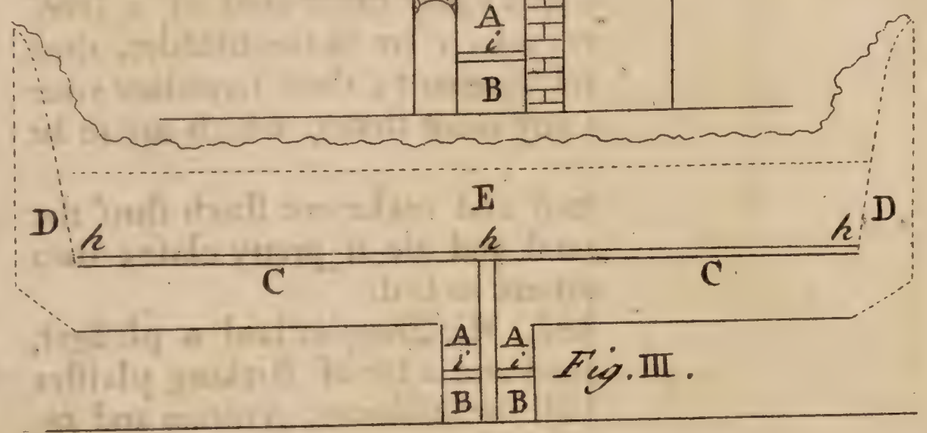


Fig. III.

Fig. XI.

Fig. V.

Fig. VII.

Fig. IV.

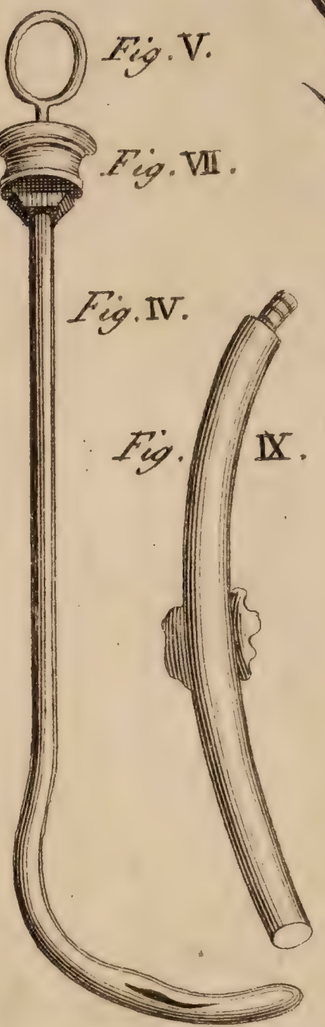


Fig. IX.

Fig. X.

Fig. XII.

Fig. VIII.

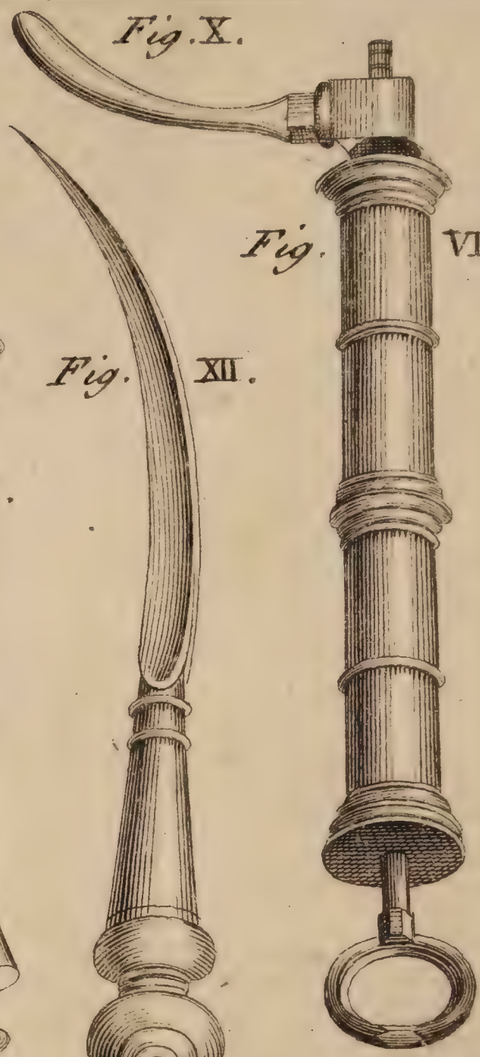


Fig. XIII.

Fig. XIV.

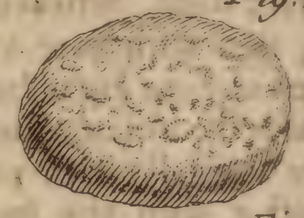


Fig. XV.

Fig. XVI.

Fig. XVII.2.

Fig. XXIV.

Fig. XXV.

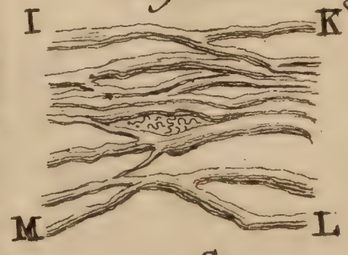


Fig. XIX.1.

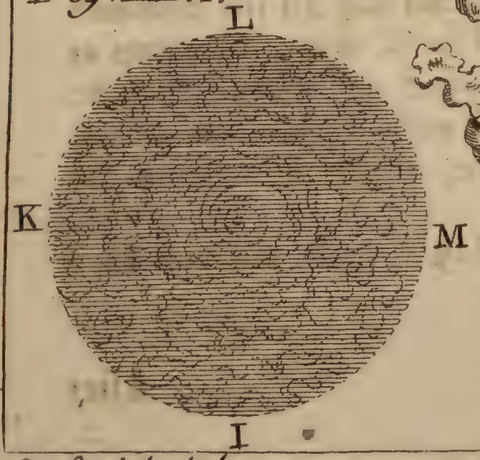


Fig. XVIII.

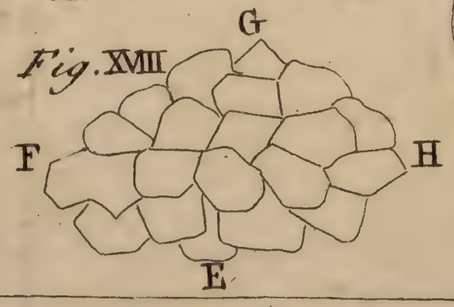


Fig. XIX.2.

Fig. XX.2.

Fig. XXI.

Fig. XXII.

Fig. XX.1.

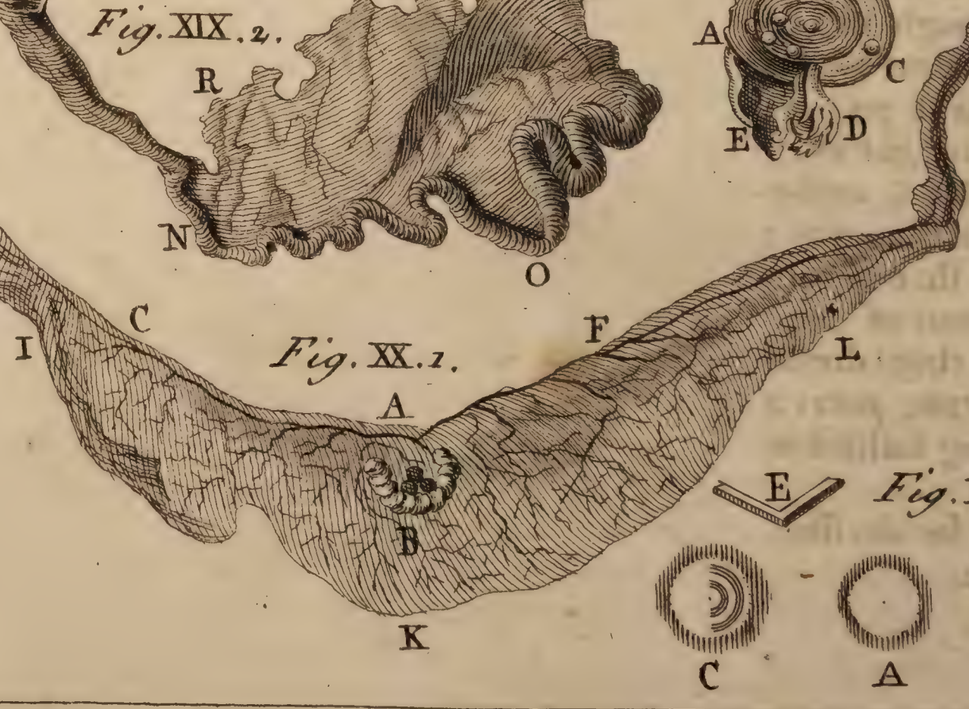
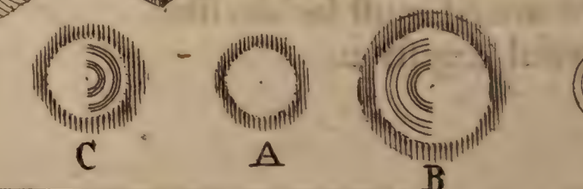


Fig. XVII.



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according to the computed bigneſs of the ſtone, down to the *os pubis* and exactly in the middle: When you are got a little more than half way thro' the abdominal muſcles, take the ſecond knife, as repreſented by Fig. 12. clap its back on the middle of the *os pubis*, then run its point down towards the *ſphincter*, till you get into the cavity of the bladder, which is diſcovered by the iſſuing out of the water; then run your knife along very quickly towards the fund of the bladder, as far as is neceſſary.

3. In extracting the ſtone, which is done thus; before you withdraw your knife, introduce the fore and middle fingers of your left hand, between the knife and the *os pubis*, into the bladder; then withdraw your knife and thruſt the fore and middle fingers of your right hand into the *anus*, and raiſe the ſtone up towards the wound, and ſo you will eaſily catch hold of it (tho' never ſo ſmall) with your fingers which are in the bladder, then extract it with the ſmalleſt end foremoſt; then introduce your fingers again, to ſee if there be any more ſtones, which are to be extracted as before.

Then take a needle and thread and make one ſtitch thro' the ſkin, in the middle of the wound and tie it pretty cloſe; then undo the ſtraps and carry the patient to bed.

The patient being put to bed, Mr. *Douglas* laid a pledget, armed with balſam over the wound and a bit of ſticking plaiſter over that; then he embrocated all the *abdomen*, *ſcrotum* and *penis*, with warm *Ol. Chamæmel.* then he applied over the dreſſing and all the *abdomen* an emollient poultice, ſpread almoſt an inch thick on ſoft flannel; then he turned a ſwath, a little broader than the patient's hand, once round him and pinned it on the poultice cloth, juſt tight enough to keep it on.

As ſoon as the patient was dreſſed, Mr. *Douglas* gave him an opiate (for nothing is ſo proper as reſt) ſuch as this ℞ *Aq. Cinnam. Hord.* ℥ii. *Laud. liq. gut* xv. *Syr. de Mecon.* ℥ii which may be increaſed or diminiſhed, as the caſe requires.

Next evening Mr. *Douglas* took off the poultice and dreſſing and cut the ſtitch; then fomented the wound and all the *abdomen* with ſtupes, wrung out of *Aq. calc.* and freſh urine, as warm as he could bear it; then dreſſed the wound as before; then he rubbed all the *ſcrotum*, *penis* and groins with *unguentum album*, to prevent their being ſcalded by the urine, which flows from the wound.

The wound muſt be dreſſed twice a day at leaſt, till you have a plentiful digeſtion.

After

After every dreſſing the ointment and oil was uſed, as before directed.

There is little variation in the dreſſing of the wound, except ing what is common in others.

The urine flows always thro' the wound, till the wound of the bladder be cured, which is ſooner or later according to the con ſtitution of the patient.

When the urine begins to come the right way, it pains and ſcalds the patients much after the ſame manner, as when they had the ſtone (which is occaſioned by the contraction of the *urethra*, that has been ſo long uſeleſs) but it never laſts above a day or two and then they make water with the ſame eaſe and freedom, as any other perſon.

The patients ſhould not be forced to go to ſtool under ſix or ſeven days, unleſs there is ſome particular reaſon for it; becauſe ſtraining to go to ſtool injures the wound.

They ſhould never be taken up, excepting to get their beds made, till the urine come all the right way, becauſe it makes them ſick and faint; and conſequently hinders the cure of the wound.

Cold is to be avoided as the peſt, becauſe it puts the patients to a great deal of pain either to ſtifle it or to cough out.

N. B. If a flexible catheter could be paſſed and kept in the paſſage without pain, it would very much haſten the cure of the wound.

Mr. *Douglas* performed the operation for the firſt time *December* 23. 1719 upon a boy between 16 and 17 years of age, and in five week time he was perfectly cured.

The ſtone was of the figure and bigneſs, as repreſented by Fig. 13.

He made the ſecond operation on the 12. of *May* 1720, on a boy of eight years of age, and in ſix weeks time he was perfectly cured.

The ſtone was of the figure and bigneſs, as repreſented by Fig. 14.

The third patient was but three years of age and was cut in *Auguſt* 1720, but died of convulſions about 15 hours after the operation.

Fig. 15. repreſents the form and bigneſs of the ſtone.

The fourth operation was made *March* 23. 1720-21, upon a boy about 14 years of age, and in four weeks afterwards he was perfectly cured.

Fig. 16. repreſents this ſtone.

In

In this patient Mr. *Douglas* made a ſmall wound in the *peritonæum*, thro' which he obſerved the guts to preſent, but he puſhed them back with his finger and then ſtitched the ſkin, and there was no farther inconveniency by it.

This high operation may be performed with equal ſucceſs on females, when the ſtone is large; but if it be but ſmall, the common way of extracting is very good.

From all which Mr. *Douglas* concludes in the words of the inimitable *Roſſetus*.

Poſt hæc nemini dubium eſſe debet, novam hanc noſtram cyſtotomiam, vetere illâ tot doctiſſimorum Chirurgorum cyſtotomiâ (tam periculoſâ, ut eam aggredi vel ipſe Hippocrates Chirurgôn Chirurgotatos metuerit) & leniorem & tutiorem haberi.

An Account of a Parhelion *obſerved in* Ireland; *by Mr.* Dobbs. Phil. Tranſ. N° 372. p. 89.

MARCH 22. 1721-22, about ½ an hour after five in the afternoon nearly; Mr. *Dobbs*, being in the fields about *Caſtle Dobbs* in the county of *Antrim*, obſerved a diſtinguiſhable parhelion, the ſun being near W. about an hour high, the wind and motion of the clouds about N. and by E. the ſky obſcured in ſeveral places with light clouds, and the ſun entering into one ſomewhat more watery, yet ſo as to diſtinguiſh its diſk: At firſt appeared below the ſun, breaking out of the cloud, ſuch rays as are uſually obſerved in an evening, in a ſky interſperſed with clouds. In a little time appeared at the ſame height with the ſun, as near as Mr. *Dobbs* could conjecture (having no inſtrument) a luminous ſpot, about four times the largeneſs of the ſun's diſk, and about 30 degrees diſtant from the ſun to the ſouthward, which was covered with the lively ſhades of red and yellow on the ſide next the ſun, and increaſed in ſplendor (ſo as ſcarce to be endured by the naked eye) till it exceeded the brightneſs of the ſun, which was then under a thin cloud, ſo as eaſily to perceive his diſk: After this had appeared about three or four minutes, Mr. *Dobbs*, perceiving it to be a real parhelion, began to look about for the halo they generally appear in; and as he obſerved ſome rays like a glory to point upwards from the ſun, he ſaw in thoſe at the ſame diſtance (being as near as he could conjecture about 30 degrees perpendicularly above the ſun) the colours of the halo appearing as in the luminous ſpot; but inſtead of finding it, as he expected, in a circle ſurrounding the ſun, it was inverted, yet not circular, but forming an obtuſe angle, the point being towards

towards the ſun. He then looked towards the northward of the ſun, and as the cloud, which was thicker on that ſide, moved ſoutherly, a luminous ſpot began to appear at the ſame diſtance from the ſun as the other, and in the ſame parallel of altitude, which had the ſame colours towards the ſun, and increaſed in brightneſs, but did not come up to that of the other ſpot, yett was as luminous, as the ſun then appeared; this ſpot was very little bigger than the ſun's diſk. As the cloud moved on, till itt came to about 60 degrees to the ſouthward of the ſun, and 30 degrees from the ſpot, at an equal height there appeared another ſpot, tinged with the colours of the rain-bow; the whole appearance laſted $\frac{1}{4}$ of an hour. The reaſon of Mr. *Dobbs* nott ſeeing the halo's, which generally appear with them, was, that there was a good deal of clear ſky above the ſun, and the cloud too thick below it.

A Fig. 17. Plate. VI. repreſents the place of the ſun, being nearly weſt about 12 or 13 degrees above the horizon, about an hour before ſun-ſet; B the luminous ſpot, being about 30 degrees to the ſouthward of the ſun, as near as Mr. *Dobbs* could compute (having no inſtrument to take the angle) and in the ſame parallel of altitude, the ſpot was not ſo well defined as in the Fig. being more imperceptibly ſhaded off in the cloud, the two ſemicircular lines next the ſun were thoſe tinged with the colours; that neareſt the ſun being of a deep ſcarlet; the inner one a deep yellow, both the colours being ſoftened, as they fell off from the ſun; all the reſt of the ſpot being an intenſe light, ſo as the naked eye could ſcarce bear it; C the other ſpot to the northward, which appeared ſome time after that, marked B, being not quite ſo large, nor the colours ſo intenſe, but diſpoſed in the ſame way, thoſe next the ſun being red, the next yellow and the reſt white; D a ſpot in the cloud, as it moved ſoutherly, till it come to about 60 degrees diſtance from the ſun, which had the colours as in the other ſpots; that next the ſun being red, the next yellow, but much fainter than in the parhelia; E the appearance of two ſegments of circles, at about the ſame diſtance from the ſun, as the parhelia, being perpendicularly above it; the colours being fainter than in the parhelia, but diſpoſed the ſame way; the lower lines next the ſun expreſſing the red and the upper the yellow.

The colours at D and E, as they were not ſo intenſe, neither were they quite ſo broad, as thoſe at B and C, the two colours being added together were about $\frac{1}{4}$ of the diſk at B, and the

colours;

colours in the ſame proportion at C; the diameter of the parhelion at B being about double the apparent diameter of the ſun, as near as Mr. *Dobbs* could compute.

The centers of the ſegments of the halo's, marked E, if not in the parhelia, were very little below them.

Below the ſun and parhelia the cloud was too thick to diſcover any thing thro' it; and above them, till near the ſegments, marked E, the ſky was ſerene and nothing obſcured; but at E, where the rays, which pointed upwards from the ſun, terminated, it appeared hazy and ſo thick as to reflect the colours.

An Account of the Particles of Fat; *by M.* Leewenhoeck. Phil. Tranſ. N° 372. p. 93.

IN a former *Tranſaction* M. *Leewenhoeck* affirmed, that the matter, called meal or flower in wheat, rye, barley, oats and in all ſorts of beans, is ſhut up, as it were, in little cells, ſeparated from each other by thin membranes, and theſe thinneſt in wheat.

In his enquiries into what is called the *perioſteum* of an ox or ſheep, M. *Leewenhoeck* often broke in pieces the fat particles thereof, and as often viewed them thro' a microſcope; a few of them he likewiſe placed upon a clean glaſs-plate and afterwards held the ſaid glaſs-plate over a coal fire or the flame of a candle ſo long, till they were all melted and reduced to a liquid matter; ſo that not only the fat, which was ſhut up in the ſkin or pellicle of the fat globules, but likewiſe the pellicle itſelf was reduced to a fluid matter; and thereupon bringing it immediately before his eye and viewing it attentively, he perceived, when the melted fat was cold, that different ſubſtances were incloſed in the ſaid fat globule; there appeared a vaſt number of exceeding ſmall coagulated particles, and the reſt of the parts, of which the fat was compoſed, lay in one ſmooth and even ſubſtance; and he has been conſidering, whether there might not be incloſed in ſuch a globule of fat, ſo many little cells and partitions, as we obſerve in a ſmall grain or ſeed; but if this be ſo, it will remain concealed from our eyes.

Having again carefully viewed theſe coagulated globules of fat, ſeveral of which form one ſmall bubble, M. *Leewenhoeck* often imagined, that he obſerved each of the ſaid ſmall particles provided with ſuch a tranſparent dent, as he before affirmed, the meal globules of wheat to be furniſhed withal: From which obſervation he concluded, that ſince we find, the great Creator of the univerſe has formed all his creatures almoſt in

the ſame manner; and that there grows no plant whatever, whoſe ſeed is never ſo ſmall, but there is incloſed in it the ſame ſort of particles, as is found in larger ſeeds; and tho' they differ in magnitude, yet that they are analogous to each other; we may eaſily imagine, that a fat globule has a bark or pellicle, as well as any ſeed, as has been often obſerved, and that they are furniſhed internally with particles, analogous to thoſe of the ſeeds, called wheat, oats, *&c.* and tho' we render fat fluid by the help of fire, yet will the ſmall particles, incloſed in the fat globules, again reaſſume their former figure, as M. *Leewenhoeck* formerly ſaid.

Nay, he fancied, tho' it did not appear to his ſight, that each fat particle is furniſhed internally with little cells, like the ſeeds or fruits of plants.

A butcher having killed a ſheep of an uncommon ſize, weighing 140 pounds, excluſive of the fat, which weighed 51 pounds; ſo that the whole ſheep weighed upwards of 190 pounds; M. *Leewenhoeck* procured a piece of the fat, that grew about the kidneys, imagining, that its fat particles would be of a coarſer grain than thoſe of ordinary ſheep; for, he often obſerved that the bigger an ox was, the larger were the fat particles, and that there are not any two of the ſaid particles of one and the ſame figure, being compreſſed by other particles with which they are ſurrounded, as M. *Leewenhoeck* frequently obſerved; ſome few of which are repreſented in Fig. 17. Plate VI. between A B C D.

Now when we meet with one of theſe little bundles of fat particles, as has frequently occurred, in which the fat particles were four times this thickneſs, M. *Leewenhoeck* imagined, that ſuch fat particles cannot be produced out of one ſingle adipoſe veſſel; but that out of ſuch a veſſel ſeveral ſmall ſprigs iſſue forth, and out of each of thoſe ſmall ſprigs proceed others ſtill ſmaller; and that out of theſe particles one large fat particle is formed like a bunch of grapes.

Now he cut off with a razor the fat in ſeveral places of a larger piece, as thin as poſſible; laying the thin ſlices upon ſeveral glaſs-plates, he put them upon a coal-fire, ſo as to cauſe them to melt; upon which immediately viewing them with a magnifying glaſs, he obſerved the pellicles or membranous coats of the fat globules, lying among the melted particles; and in the ſaid particles there was nothing to be perceived but a limpid matter, ſurrounded with ſmall air-bubbles: But when the fat was congealed, but very little of the membranes could be obſerved

obſerved, becauſe they were covered with the particles of the fat, with which theſe pellicles or membranes had been before filled.

Fig. 18. repreſents between EFGH a few of theſe pellicles of the fat globules. During the ſaid obſervation he attentively fixed his eye upon the fat particles of the ſheep, that had been melted and were again coagulated; and he could not but judge, that the ſaid particles, which were exceeding ſmall, were analogous to that internal matter, with which ſome of the ſmalleſt little ſeeds are furniſhed; and in a great many of theſe exceeding ſmall particles, he could in clear weather diſcover ſome tranſparency.

Moreover, he cut ſlices of the fat as thin as poſſible; yea ſo thin, that five or ſix of them did not weigh a grain, and he put them into a little water, in order to try whether he could thereby make any farther diſcoveries, with reſpect to the ſmall particles of fat; but to no purpoſe, only he obſerved floating upon the water very ſmall particles of fat, that were coagulated in a ſpherical figure, and the very largeſt of them was no bigger than a grain of ſand: He placed theſe particles upon a glaſs-plate, and viewing them with a microſcope, he obſerved the abovementioned figure, as plain as before; and other fat particles ſeemed to be of a different figure.

Fig. 19. repreſents between IKLM one of the ſaid particles, coagulated on the water, that was not very conformable with the other melted fat particles; for, all the particles did not melt nor were all extracted by the water, but coagulated thereon in ſmaller and larger globular particles; and upon taking out the remainder of the thin ſlices of fat, that float upon the water, and viewing them with a microſcope, he found that ſeveral of the fat particles appeared entire to the eye; and whereas they were before very ſmooth and even in their ſides, they were now changed into rough and uneven particles; ſo that one would be apt to think, that there were two different ſorts of particles in the fat, and that one ſort melted more eaſily than the other.

Now in order to get theſe melted particles of fat out of the water, without altering them, M. *Leewenhoeck* made uſe of a round glaſs, with which he ſkimm'd the ſurface of the water; by which means ſome of the coagulated particles ſtuck to the glaſs: Moreover, he again melted ſome of the fat particles, which had been coagulated upon the water,

over a coal fire, as they lay in the water; and when they were again coagulated, upon viewing them with a microscope, he found the small fat particles still smaller than those that were melted out of the water.

In this last observation he was surprised at the inconceivable number of veins and membranes, that were diffused thro' the fat, as also of the separated fat particles, involved in their several membranes.

After thus cutting off with a pair of sciſſars some pieces of the caul, or net spread over the hinder quarter of a sucking lamb, upon which there was little or no fat, and placing them before a microscope, he observed again, that the fat particles, where there were very few of them included between the membranes, were of a more globular figure than in other parts, where a good many lay together, and that in other places they were pres's'd or bruis'd, which he supposes was occasioned by the butcher's squeezing the caul in that place with his fingers; and in another place the fat particles had been so torn in pieces, that he could observe nothing remaining but the skins of the fat globules.

Moreover, he observed that the fat particles had such a pinch or dent in them, as M. *Leewenhoeck* shewed were in the globules of flower of wheat; from which he was confirmed more than before in his opinion, that the fat globules might be separated entirely or in part, from the pellicle with which they are surrounded, by opening the dents without breaking the pellicle.

Then he took off the thin membranes, which encompassed the fat particles, and viewing them with a microscope, he observed, that the fat particles had imprinted a roundish figure on the membranes, inclining to a hexangular one, which was an agreeable sight; but in other parts they were of an oval figure.

Moreover he took a flat fish, called a *plaise*, and taking off the fat which adhered to the vessels, or bones, he viewed it with a microscope and observed, that the fat particles were of several sizes; and some so small, that he judged, that 100 of the smallest were no bigger than one large fat globule; and moreover he observed, that several of the fat globules had such a dent in them, as we find in the meal or flower of those little white beans, called *French* or kidney beans.

M. *Leewenhoeck*, taking a little of the fat of a perch, which was 9 or 10 inches long, and viewing it with a micro-

scope;

ſcope, could not diſcover any ſmall particles therein, nor any internal dent, as he had obſerv'd in the fat of a ſmall plaiſe.

After that the fat of a perch had lain an hour or two upon the glaſs, he view'd it again and obſerv'd, that the particles were become ſmaller; and that the pellicle of the fat particles, which was ſtill beſet with ſome, was ſhrunk or wrinkled, as it were; and the fat that had burſt out, lay about the fat particles, and was ſo fluent and tranſparent, that no parts could be diſcover'd therein.

From this obſervation M. *Leewenhoeck* began to think, whether each of theſe fat particles was not furniſhed with an orifice or hole, out of which the fat might be protruded at all times, as often as the parts of the fiſh ſtood in need of nouriſhment, without laying quite open the pellicle of the fat globules; for, we conſtantly find, that when the eggs of the perch, called the roe, increaſe in bulk, its fat decreaſes; yea, in ſuch a manner, that when the ſaid eggs are arriv'd at their utmoſt bigneſs, there is ſeldom or never any fat to be obſerv'd on the inteſtines of the fiſh.

An Account of a new Iſland rais'd out of the Sea near Tercera; *by Mr.* Forſter. Phil. Tranſ. N° 372. p. 100.

JOHN Robinſon, maſter of a ſmall pink-ſnow from *Piſcataqua* in *New England*, arriv'd at *Tercera Dec.* 10 1720; at 17 leagues diſtance, bearing S. E. from the ſaid iſland he obſerv'd a fire break out of the ſea; *Dec.* 18. he got under ſail at 12 o'clock at night and ſtood from *Angras* S. E. The next day at 2 o'clock in the afternoon, he made an iſland all fire and ſmoke; he continued his courſe, till the aſhes fell all night on deck like hail or ſnow; he bore from it, the fire and ſmoke roar'd like thunder or great guns; at break of day he ſtood towards it again; at 12 o'clock he had a good obſervation, two leagues ſouth from it; he ſail'd round it and ſo near, that the fire and matter it threw out, had like to have done him damage; in which conſternation all betook themſelves to prayers, being in danger of driving aſhore; then a ſmall gale ſprung up at S. E. and carried them clear to their great joy; the breeze was accompanied with a ſmall ſhower of rain, which caus'd a great duſt to fall on deck; with the ſaid breeze he ſtood away for *Tercera*: The Governor inform'd him, that the fire broke out *Nov.* 20. 1720. in the night, and that the prodigious noiſe it made, caus'd an earthquake, which ſhatter'd ſeveral houſes in the town of

Angra

Angra and places adjacent. Prodigious quantities of pumice-ſtones and half broil'd fiſh were found floating on the ſea for ſeveral leagues round the iſland, and abundance of ſea-fowl hovering about it.

Mr. *Forſter* was inform'd by an acquaintance, that in his paſſage from *Cadiz* to *London* the latter end of *April* 1721, he obſerv'd the ſea from cape *Finiſterre*, almoſt to the chops of the channel, cover'd with pumice-ſtones, ſome of which he gave him.

The Effects of a violent Shower of Rain *in* Yorkſhire; *by Mr.* Ralph Thoresby. Phil. Tranſ. N° 372. p. 101.

THE daſhing (as they call it) of two large watery clouds upon the hills, occaſioned an inundation at *Riponden* near *Halifax*; whatever was the more immediate cauſe thereof, the effects were diſmal and ſo ſudden, that tho' it happen'd in the day-time, *May* 18. 1722, between the hours of four and five o'clock in the afternoon, the people could not ſave their lives. By the modeſteſt computation the *Beck* was rais'd two yards at leaſt in perpendicular height above what was ever known before, which may be eaſily conceiv'd by the ſituation of the place, implied in the termination *den*, which ſignifies a deep valley between pretty ſteep hills on each ſide. *Fontes in convallibus Pſ.* 103. 2. is in the *Saxon* verſion render'd pillar on denum; and *Valley of Tears*, *Pſ.* 83. 6. dene teopa. Several houſes, four mills, (according to ſome ſix) 9 ſtone and 10 or 11 wooden bridges were carried down by the flood, and the wheels, dams and ſluices (or *goits* as they call them in *Yorkſhire*, from the verbal noun geotan *fundere*) of moſt of the mills, that were left ſtanding, broken and damaged, and a great deal of cloth gone: Fifteen perſons were drowned; ſeven out of eight in one houſe were either kill'd by the fall of it or drowned.

The rapidity of the torrent was ſo violent, that it took down the north ſide of *Riponden* chappel and carried off moſt of the ſeats. The Rector of *Caſtleford* inform'd Mr. *Thoresby*, that ſeveral goods were carried down ſo far, tho' upwards of 20 miles off. It tore up the dead out of their graves: It ſwept away all the corn-land as deep as the plough had gone: Some perſons ſav'd themſelves by forcing a way out at the roofs of their houſes, and ſitting upon the ridges, till the flood abated.

That

That day Mr. *Thoresby* had a ſmart thunder-ſhower upon the moor, as he was coming home.

An Account of a new Experiment made with the Blood of a Perſon dead of the Plague; *by Dr.* Couzier. Phil. Tranſ. N° 372. p. 103.

ON the firſt of *April* 1722, Dr. *Couzier* took a quantity of blood from the body of a perſon dead of the plague, and mix'd it with warm water; which mixture he attempted to inject into the crural vein of a dog, but the end of the ſyringe being two large to enter the vein, the experiment did not ſucceed.

Upon this he laid ſome of the ſame infected blood upon the wound, and cover'd it with a dreſſing, which the dog got off in the night; next morning he found the dog had lick'd his wonnd, and that he refus'd his food; towards night he began to bemoan himſelf, and gave ſigns of approaching death; next morning he found him dead, the wound being conſiderably ſwell'd and gangren'd, as alſo the edges round the ſwelling.

Upon opening the body, the liver was found ſomewhat larger than uſual, with ſpots of a livid purple, as in the bodies of perſons dead of the plague: In the ſtomach was found a quantity of black coagulated blood, of the ſize of a hen's egg; this in all likelihood was what he had ſwallow'd upon licking the wound. The heart was very large, with a black grumous blood in the ventricles, and the auricles were turn'd blackiſh and gangrenous.

An Account of an Experiment made with the Bile of Perſons dead of the Plague; *by Dr.* Deidier. Phil. Tranſ. N° 372. p. 105.

TWO dogs were made to ſwallow a pretty large quantity of the bile taken from the bodies of perſons dead of the plague.

Upon this the dogs appeared heavy and melancholy, refus'd their food, and made water very often, eſpecially when they were any ways diſturb'd; their urine was thick, and very fetid, and their groſs excrements tinged with the black and greeniſh bile, they had ſwallowed. But in a few days thoſe accidents went off, and the dogs perfectly recover'd.

Obſer-

Obſervatiens on a Fœtus, *and the Parts of Generation of a* Sheep; *by M.* Leewenhoeck. Phil. Tranſ. N° 373; p. 151.

AN ewe, which within two years time had twice lamb'd, happen'd to be cover'd by a young ram about 20 weeks old; about five days after the ewe was kill'd; and out of her belly 28 pounds of fat were cut; but obſerving, upon opening her, that the *uterus* was four times bigger than ordinary, the butcher brought it, together with the *ovaria* to M. *Leewenhoeck*, aſſuring him, it was not quite five days ſince the young ram had cover'd the ſheep, and that there was no other ram thereabouts.

M. *Leewenhoeck* began firſt to try to penetrate with the point of a ſmall pair of ſciſſars into the *uterus* from the *vagina*; but he found it ſo cloſe, that he could not; therefore, he cut a piece off from the *uterus*, out of which ran a clear water, and within it lay the *fœtus* with all its coverings; he ſpread this upon the back-ſide of a china tea-diſh, and finding that it ſtill contain'd more water, he made a ſmall inciſion to drain it, and to let it dry, that he might obſerve it the better; he could plainly ſee the *vertebræ* of the neck and back, as alſo the joints of its ſhort tail; he likewiſe thought he ſaw the eyes; but when it was quite dry, he could not obſerve its back-bone ſo well as before, when it was moiſt, tho' the deſigner ſaw the bones of the back very diſtinctly. M. *Leewenhoeck*'s deſign in drying it was to cut it in ſmall ſlices, in order the better to obſerve the internal parts; for, it was ſo exceeding ſoft and tender, when moiſt, that with the leaſt touch its parts would be diſorder'd and confounded: Therefore, he cut this *fœtus* into 15 ſlices and obſerv'd them with a microſcope, but could not be very poſitive about what he ſaw; he imagin'd he ſaw the inteſtines, as alſo the bladder; and coming to the breaſt, he fancied he ſaw the heart; but he obſerv'd (which was an agreeable ſight) two blood-veſſels, that lay near each other in the brain, and how they were ſpread into branches.

Fig. 20. Plate VI. repreſents the *fœtus*, as it lay in its integuments; A B the *fœtus*; A C D E I K and A F G H L K. the membranes in which it was involv'd, in the manner M. *Leewenhoeck* had ſpread and dried them, wherein the blood-veſſels are delineated as much as poſſible: As to blood-veſſels, they have, as he has often ſaid, no termination; beſides,

beſides, they gradually become ſo exquiſitely fine, that the blood which paſſes thro' them, can exhibit no red colour to our eyes; ſo that there is no tracing them, when entering into the veſſels that return the blood back to the heart, excepting in live animals, where one may obſerve the blood enter into the returning veſſels: Before the butcher brought M. *Leewenhoeck* this *uterus*, he ſqueez'd it between his fingers, and told him he could feel nothing therein; and M. *Leewenhoeck* ſuppoſes he had done this ſeveral times, by which means he tore off the veſſels, by which the *fœtus* was faſtened to the *uterus*; and this he takes to have been the reaſon, that upon opening the *uterus* the *fœtus* with its coverings came forth ſo eaſily.

MNOP Fig. 19. repreſents the *Tuba Fallopiana*; at P is the imaginary orifice, which is thought to ſuck the egg from the *ovarium*, according to the old abſurd notion; at M is ſhewn, where the *tuba* increaſes in bulk, and at QR the fleſhy ſubſtance, which M. *Leewenhoeck* cut away from the *uterus*. He then alſo had cut off the pretended *ovaria* and *ova*, which latter were much too big to paſs upon conception thro' the *Tubæ Fallopianæ*: He therefore, took the length of the *fœtus* with a pair of compaſſes, and meaſur'd it upon a divided braſs ruler, and this he alſo did as to its breadth; he then took the mean number between theſe and multiplied it twice by itſelf, to bring it to a cube number; next he took the length of the axis of an *ovum*, as it lay in the *ovarium*, inclos'd in its membranes, and taking the cube of that length and dividing one cube number by the other, he found, that ſuch an *ovum* was about 7 times bigger than the *fœtus*, notwithſtanding it had had near five days growth.

M. *Leewenhoeck* ſhew'd this *fœtus* with its integuments to two phyſicians and a ſurgeon; as alſo the pretended *ovaria*, and they agreed, that not one *ovum* was miſſing out of the *ovaria*. Upon aſking them how it was poſſible ſuch an *ovum* could paſs thro' the *Tuba Fallopiana*; one anſwer'd, that the *ovarium* was quite out of the queſtion, and that it was nothing but ſome fleſhy ſubſtance: But the other ſaid, that notwithſtanding this, all animals proceeded from an egg, and the laſt told M. *Leewenhoeck*, that he believ'd the *tuba* to be neither that of a ſheep or lamb; but M. *Leewenhoeck* ſhew'd them it was from an ewe, that had lamb'd twice, and yet that the *tuba* was neither thicker nor wider than that of a lamb.

After keeping these *ovaria* for some days, by which means they were pretty much shrunk in drying, M. *Leewenhoeck* had them delineated, in order to observe the bigness of the *ova*.

A B C D E Fig. 20. represents the *ovarium*, which was on that side of the *uterus*, in which the *fœtus* had lain: It is to be observ'd, that the *uterus* of a sheep is divided by a membrane, so that the *fœtus*'s cannot touch each other; D E at the part where it was fastened; in this *ovarium* may be observ'd a round protuberance, beset with several others, and call'd an *ovum*: This *ovarium* is not represented so large, as it was, when M. *Leewenhoeck* cut it off from the parts it grew to; there was besides, at one side of that *ovarium*, a large round body, grown to the *ovarium*, which also seem'd to be an *ovum*; as F G Fig. 21; on which there appear'd several other small round bodies, protuberant therefrom: Now on the other side of the *uterus*, there was a large fleshy prominent *ovum* (as it is call'd) which might plainly be observ'd without a microscope, whose bigness is likewise represented at H I Fig. 22. upon which may likewise be observ'd a small round body, out of which other round bodies still smaller, appear protuberant. After he had quite dried these *ova*, represented in Fig. 20, 21, 22, he still observ'd more and more of the prominent round bodies thereon; insomuch that upon one of them, he told 16 round little bodies, some of which, by losing all their moisture, were sunk in and had a dent in the middle: Furthermore he cut these *ova* with a very fine sharp knife into thin slices, and then viewing them with a microscope, he observ'd blood-vessels therein, and likewise other sorts of vessels, which he did not take for blood-vessels; and among the rest one so big, that a hair of the head might enter it; besides a great many others exceeding small.

After several observations he could conclude in no other manner than that the pretended *ova* consisted of nothing but vessels, and that the superfluous moisture, convey'd to these *ova*, did not circulate (excepting only what was in the blood-vessels) and by overcharging the vessels rais'd them into these small protuberances, and sometimes bursting them, did thereby leave a dent in the middle; which dent having been observ'd by some persons, they firmly believ'd, that that was the place where the *ovum* was suck'd out, from whence sprung the *fœtus*.

Had

Had M. *Leewenhoeck* got this *uterus* without being ſqueez'd, he does not queſtion, but that he might otherways have diſcover'd all the members of the *fœtus*, ſince he could plainly obſerve its back-bone even with the naked eye, tho' not quite of five days growth.

After this it cannot be pretended, that the *fœtus in utero* is at firſt no other than an unform'd maſs.

Obſervations on the Callus *of the Hands and Feet; by the Same.* Phil. Tranſ. N° 373. p. 156.

IN *September* 1719, M. *Leewenhoeck*, feeling an acute pain at the joint between the foot and little toe, which he imagin'd to be owing to the more than uſual thickneſs of the *callus* or hard ſkin upon that part; he caus'd his ſervant, partly with his nails and partly with a penknife, to take off that hard ſkin, and let it fall upon a blue paper.

This *callus* was compos'd of little ſcaly ſhivers, lying upon each other; and the whole piece was as large as a ſmall nail of a man's hand.

He view'd the ſaid ſhivers thro' a microſcope, but could not ſatisfy himſelf; becauſe they lay ſo irregular upon each other.

Moreover, taking a little bit of the aforeſaid *callus*, he laid it on a clean glaſs-plate, ſteep'd it in pure rain-water, and gently dividing it with a piece of a quill, he was amaz'd to ſee into what a vaſt number of particles it ſeparated, and that with as much readineſs, as if they had never been join'd.

Afterwards he took two or three of the ſaid particles, ſeveral of which were of the figure of a weaver's ſhuttle, broad in the middle and pointed at each end with a line in the middle, like thoſe on the outſide ſkin of fruits or of the human body, but generally irregular; they were very thick in proportion to their bigneſs: Having laid two or three of the ſaid particles upon a clean glaſs, and putting a drop of water to them, as large as a coarſe grain of ſand, and dividing the ſame as much as poſſible; upon viewing the divided particles thro' a microſcope, he was ſurpris'd at the prodigious number of exceeding ſmall particles, which occur'd to his ſight and were of the ſame figure, as above-mentioned.

Moreover, he took ſome of the thickeſt pieces of the horny ſkin, but not half ſo thick, as the back of a ſmall knife, and ſlitting them into as thin ſlices as poſſible, he placed them

upon a clean glaſs, in order to diſcover the exceeding thin particles that lay upon each other; and having moiſtened them, they ſpread themſelves out farther, and when they became dry again, they ſeparated into ſeveral parts; and he likewiſe obſerved, that each of theſe ſeparated parts were compoſed of ſeveral thinner particles, lying on each other.

In order to have a clearer notion of the contexture of thoſe particles, by which the ſkin of the hands and feet of ſuch, as are inured to hard labour, or walk much, increaſes in thickneſs and is ſurrounded, he cauſed a ſmall portion of the aforeſaid ſeparated particles to be delineated, as A B C D Fig. 23. Plate VI. tho' they were not all ſo exact and compleat, as repreſented in the Fig. and according to the beſt of his obſervation, they had all been ſeparate pieces and none of them united to each other.

Again he placed before a microſcope ſeveral ſmall pieces of this hard ſkin, he had cut off at the thickeſt part thereof, and moiſtening them with fair rain water, he put them upon the glaſs-plate, by which means they were pretty much dilated; and being dried, they ſhrunk again and thereby appeared in ſeveral oblong particles, each of which ſeemed to conſiſt of other oblong particles, as repreſented at E F G H Fig. 24; ſo that E H or F G was the thickneſs of that piece of thick ſkin, which he had cut thro'.

From this obſervation, he conſidered, whether one of theſe long ſtripe-like particles, ſuch as appeared to the eye at E F or H G, might not be the thickneſs, which the horny ſkin had acquired in the ſpace of a month; and whether the very thin particles, which appeared in ſuch a ſmall ſtripe, were not the accretion, the hard ſkin received in a day: The laſt mentioned ſmall piece repreſented Fig. 24. was not ſo large to the naked eye, as a common grain of ſand. And whereas he placed before a microſcope another very ſmall piece of ſkin, ſomewhat thinner than the former, he could perceive the exceeding thin particles, repreſented by I K L M Fig. 25, which were the *ſtrata* or beds, in which the horny particles of the ſkin lay, and ſo compoſed its thickneſs.

For his farther ſatisfaction he cut out of the hand of a maſon, a laborious workman, where it was moſt thick and brawny, two bits of the hard ſkin; and then ſlitting it into ſmall ſlices, he obſerved eaſily enough the thin particles lying upon each other, but could not ſeparate the ſmall ſcales, of which each little *ſtratum* of the hard ſkin conſiſted; becauſe,

becauſe, as M. *Leewenhoeck* imagined, the particles of the ſaid ſkin, thro' the hard working of the man, were ſo preſs'd upon each other and ſo cloſely joined, that they could not be ſeparated.

Now as the hands of maſons or bricklayers are often covered with the ſharp ſalts of the chalk or lime, which might prevent the ſeparation of the very ſmall parts of the hard ſkin; M. *Leewenhoeck* pared off ſome of the brawn from the hands of a carpenter, a diligent workman, and he found them as ſoft in the palms, as if he had never been uſed to labour; upon which M. *Leewenhoeck* aſked him, whether he did not waſh his hands very often; to which he replied, 10 times a-day at leaſt. M. *Leewenhoeck* cut out two pieces of horny ſkin from the hands of a ploughman, and theſe he cut into little bits; but they were ſo hard, that a ſharp knife, which he uſed, got ſeveral notches in doing it; and he obſerved, that the uppermoſt part of the ſkin was full of little rents, and all the *ſtrata* preſſed ſo cloſely together, that he could make no diſcoveries therein, only that the little beds lay upon each other, and that the thickneſs of the ſkin conſiſted thereof.

He put the two pieces of hard ſkin into warm water, in order to ſoften them and ſeparate the parts from each other; but could not do it, becauſe they were ſo ſtrongly joined together.

In waſhing his hands, M. *Leewenhoeck* ſeveral times obſerved, that when he rubbed his palms ſtrongly againſt each other, with very little water between them, ſome particles would be rubbed off from the ſkin, and continue between his hands.

For his farther ſatisfaction in this matter, he put one of his fingers into fair rain water, and with that waſhed the part of his thumb, which is joined to his hand; after which he rubbed both finger and thumb hard againſt each other; then he gently ſcraped with a penknife the matter he had looſened from the ſkin by moiſtening and rubbing it; and taking off a little of that which ſtuck to the knife, he put it upon a clean glaſs-plate, and ſetting it before a microſcope, he ſaw with aſtoniſhment the great number of particles of ſkin, which lay ſcattered upon the glaſs,; but more irregular than thoſe he ſeparated from the brawny ſkin of his foot, which were not very cloſely preſſed together, becauſe he did not walk much.

After

After this he moiſtened the back of his hands, and then rubbing them 10 or 12 times againſt each other, he ſcraped off very gently with a penknife ſome of the matter he had looſened by rubbing; and placing it on a clean glaſs, he viewed it with a microſcope, after having ſeparated the particles of the ſaid matter from each other with a little water, and he diſcovered a vaſt many ſmall ſcales which come off our hands.

As the ſkin of his hand was in no part thicker than that upon his thumb next the nail, having chiefly uſed his thumbs in the examination of microſcopical objects, he moiſtened one of his thumbs a little and rubbed the other againſt it, and placing before a microſcope the matter, thus rubbed off, he obſerved ſuch a prodigious number of particles (like thoſe repreſented by A B C D Fig. 23. but all irregular) as no man can conceive without having ſeen it.

Now as we find, that ſuch a quantity of particles is ſeparated from the hands, and daily renewed in a well conſtituted body, we muſt conclude, that in our bread we eat ſeveral of the ſaid particles thus rubbed off, and that they turn to nouriſhment; and M. *Leewenhoeck* is of opinion, that there is hardly any food prepared for us, eſpecially ſuch as paſſes pretty much thro' the hands, but that ſome of the particles thus rubbed off are mixed therewith; eſpecially kneading dough from meal or flower; and ſtill more, when the bakers knead with their feet, as in making rye-bread.

Since theſe obſervations concerning rubbing of his hands, M. *Leewenhoeck* took more notice thereof, when he waſhed and dried them than he did before; and he was ſurpriſed at the great number of particles that daily ſeparated from his hands and grew on them again; and at the particular proviſion, made for producing theſe particles in the palms of our hands and ſoles of our feet; whereas we do not by far meet with ſuch a quantity of particles, conſtantly produced in other parts of our bodies: For, if we obſerve ſuch as work much with the back of their hands, we ſhall not meet with any of that hard ſkin abovementioned, but only a kind of tumour or riſing, as the dry-ſheerers, or ſuch as dreſs cloth, have upon their left hands.

In ſhort, the manner of the production of theſe ſmall particles will be a myſtery to us, tho' our hands and feet muſt be fortified with ſuch a matter, to enable them to ſupport all that force and preſſure, they are obliged to undergo.

Of

Of the Motion of running Waters; *by Dr.* Jurin. Phil. Tranſ. N° 373. p. 179. *Tranſlated from the* Latin.

S. *Michelotti*'s animadverſions in his book *de ſeparatione fluidorum in corpore humano*, on a diſſertation of Dr. *Jurin*'s on the *Motion of running waters*, publiſhed in *Phil. Tranſ.* N° 355, are partly owing to his not throughly underſtanding the drift of that diſſertation and partly that ſome things are not put in ſo clear a light as they are capable of: To obviate which, the Dr. firſt explains, what is to be underſtood by *the motion of water, running out at a hole in the bottom of a veſſel*: For, there is a wide difference between the motion, or the quantity of motion of water, running out at a hole in a veſſel (which motion is in a compound ratio of the quantity of water running out at the hole in any given time, and of the velocity, with which it runs out) and between the motion of the whole quantity of water or cataract of water, deſcending within the veſſel towards the hole, and immediately about to flow out; this motion being in a ratio of the ſum of all the products of each particle of water, conſtituting the cataract, multiplied into their reſpective velocities. The Dr. obſerving that one of theſe motions was often taken for the other, had a mind to illuſtrate the latter in his ſaid diſſertation, and bring it to a calculation, and apply it to the fluids in animal bodies.

Since, therefore, this motion was what the Dr. always meant by *the motion of running waters*, as plainly appears by all his propoſitions, he thought he might juſtly alledge, *that this motion had not hitherto been defined by any one, as far as he knew*; no mathematician having even ſo much as hinted at it: Wherefore, the Dr. is ſurpriſed, that neither *Michelotti* nor *John Bernouilli* were aware that in the preface to that diſſertation (ſo often cited and ſo much cenſured by *Michelotti*) he did not ſo much as mention the velocity, with which water runs out at a hole, much leſs the velocity determined by *Bernouilli*.

In order to define the aforeſaid motion, the Dr. needed no other than his third general *Theorem*: But ſince he thought, the property of the *Newtonian* hyperbolic curve, in which Sir *Iſaac Newton* forms the cataract of the deſcending water, not unworthy the conſideration of Geometricians, he would by the bye premiſe ſome things about it, as taken from *Prop.* 36. *lib.* 2. *Princip. Mathem. Philoſ. Nat.*

For,

For, it is plain to any one, who attentively conſiders Sir *Iſaac Newton*'s propoſition, that ſuch a cataract ſhould be formed by water deſcending freely, and accelerated in the manner of all other heavy bodies, without any other water ſurrounding it. And even if the cataract of water be ſurrounded with a hollow cruſt of ice, exactly anſwering to its figure, and by reaſon of its extreme politure making no reſiſtance thereto; the cataract of water will not in the leaſt preſs upon the ice, but only touch it and deſcend very freely; whence no alteration will be produced either in the figure, or velocity of the deſcending cataract. But if the circumambient ice be diſſolved, there is no manner of occaſion for ſuch a ſtrong battery as *Michelotti* p. 128, 129, 130, and likewiſe *John Bernouilli* have raiſed to break down the Dr's ſlender cataract; ſince Sir *Iſaac Newton* himſelf has quite diſſolved it, when he ſays, *Princip.* p. 304. *Liqueſcat jam glacies in vaſe*, &c.

The Dr. does not deny, but that there is ſome difference between the caſe, as laid down by Sir *Iſaac Newton* and by himſelf; for, the cylinder of ice, which the former ſuppoſes to deſcend with a given uniform velocity and to diſſolve, as ſoon as it touches the ſurface of the water, contained in the veſſel, to the end, that the veſſel may be always kept equally full, the Dr. has omitted and inſtead thereof has ſuppoſed an infinite ſurface of water, that by that means he might repreſent the whole ſolid or hyperbolic cataract: Yet this poſition alters nothing either in the velocity or motion of the running water.

What S. *Michelotti* ſays, p. 127. *that the Dr. begs the queſtion*; and a little lower, *that the queſtion, therefore, ceaſes and that the whole demonſtration becomes an hypotheſis*; this the Dr. does not underſtand: For in *loc. citat.* the queſtion was not about the velocity of the effluent water, nor was there any demonſtration adduced to prove that velocity; but the only thing the Dr. intended was from that velocity, to deduce the equation of the *Newtonian* hyperbolic curve; for, the Dr. had already determined the velocity of the effluent water, or rather aſſumed it, to wit, by ſuppoſing what Sir *Iſaac Newton* had done, *viz.* that water by the force of gravity falls freely and in falling is accelerated.

Here the Dr. takes notice of an error, *Michelotti* p. 112, 113, would raſhly faſten on Sir *Iſaac Newton*, *Huygens* and Mr. *Keill*; *viz.* that they have ſuppoſed the force, by which the

whole

whole motion of the effluent water may be produced, equal to the weight of a cylindrical column of water, whoſe baſe is the hole, and height, double that of the water, contained in the veſſel: This Sir *Iſaac Newton* had briefly but perſpicuouſly demonſtrated in the ſecond *Coroll.* of the aforeſaid *Prop.* And another demonſtration of it might be deduced from conſidering the entire hyperbolic cataract, which is equal to this cylinder and whoſe entire weight is ſpent on the deſcent of the water; but that is unneceſſary, ſince the ſame thing follows very evidently from *Bernouilli*'s own propoſition, which *Michelotti* ſo often commends and ſo ſtrenuouſly defends. This will eaſily appear, if laying aſide for a little the conſideration of the column of water, incumbent on the hole, and making the calculation, he would, from the maſs of water, running out at the hole in any given time, and from the velocity, with which *Bernouilli* has determined water to run out, determine the motion of the water and then find the weight, which, by the force of gravity, falling freely in the ſame given time, may produce the ſame quantity of motion; he will find this weight equal to that of double the column of water, incumbent on the hole, juſt as Sir *Iſaac Newton* had determined it in the aforeſaid *Corol.* But the ſame weight, ſuſpended at one arm of a balance, will be kept in *æquilibrio* by the *impetus* of the water (at its very firſt efflux out of the hole) impinging in a continued ſtream upon the other equal arm of the balance and falling down, immediately after the impulſe; which will eaſily appear upon making the calculation.

Dr. *Jurin* would here alſo remove a prejudice *Michelotti* p. 113. and others labour under: Sir *Iſaac Newton* had demonſtrated *Prop.* 37. *lib.* 2. *Princip. firſt edit.* that water runs out at a hole in the bottom of a veſſel with that velocity, with which it might riſe to half the height of the water, contained in the veſſel. Experience is ſaid to contradict this, by which it is diſcovered, that the effluent water riſes to the whole height; and Sir *Iſaac Newton* himſelf in the ſolution of the ſame problem *Prop.* 36. *lib.* 2. *the ſecond edit.* aſcribes to the water that velocity, with which it might riſe to its whole height; conſequently, he ſeems to contradict himſelf: But if this matter be accurately and judiciouſly conſidered, it will be found to agree very well both with Sir *Iſaac Newton*'s firſt and ſecond ſolutions, and likewiſe with experience; for, in his ſecond ſolution, he ſuppoſes the jet of water narrower in

diameter, at a ſmall diſtance from the hole, than in the hole itſelf, in the ratio of 21 to 25; therefore, the ſection of the jet at that diſtance is to the hole itſelf, as 21×21 to 25×25, that is, as 1 to $\sqrt{2}$ nearly: And ſince the ſame quantity of water flows in a given time, either thro' the ſection of the hole, or that of the contracted jet, and conſequently, the velocities of the water in theſe ſections are reciprocally as the ſections themſelves, the velocity of the water in the hole will be to the velocity of that in the contracted jet, as 1 to $\sqrt{2}$; conſequently if the velocity in the contracted jet be ſuch, as that the water can riſe to the whole height of that, contained in the veſſel, the velocity of the water will not be greater in the hole itſelf, than what can raiſe it to half the height: Theſe two ſolutions therefore are conſiſtent with each other and experience: For, if by either of the ſolutions, from the given velocity, with which water is ſupppoſed to paſs either thro' a hole, or contracted jet, by calculation, the quantity of water to run out be found; the ſame will be found to agree nearly with the quantity of water, diſcovered by experiments; and even Sir *Iſaac Newton*'s own experiment, taking a hole, whoſe diameter is $\frac{5}{8}$ parts of an inch, agrees with this calculation; as alſo ſeveral other experiments with holes of ſmaller diameters, made at *London*: It is true, the experiments made by the accurate *Polenus* differ ſomewhat from theſe; but yet they give a leſs quantity of water, than according to this calculation, and never a greater, becauſe, probably, the veſſels were narrower in proportion to the ſize of the holes.

There ſtill remains one animadverſion more, or rather ſcruple p. 101, 102, ariſing hence, that in *Corol.* 17. *Theor.* 3. of the abovementioned diſſertation, Dr. *Jurin* had ſuppoſed the motion or *impetus* of the blood to be greater in all the capillary arteries taken together than in the *aorta* itſelf: To explain this, *Michelotti* would faſten on the Dr. an hypotheſis of a greater denſity of blood in the capillary arteries than in the *aorta*. Dr. *Jurin* diſowns any ſuch hypotheſis, having deduced the *Corol.* from the foregoing *Theor.* which treats of the motion of water running thro' any full pipe: Whence it appears, that the blood is no otherways conſidered in his *Corollaries*, than as it is fluid, and reſembles water: But *Michelotti*'s ſcruple appears to proceed from his taking the *impetus of the blood to be the quantity of its motion, produced by multiplying the velocity into the maſs, running thro' in a given time;*

time; which is quite different from Dr. *Jurin*'s motion or *impetus*; he having in that *Theorem ſuppoſed it equal to the motion of a maſs of water, which runs out of a pipe in any given time, and whoſe velocity is ſuch, as in the ſame time to run over a ſpace equal to the length of the pipe.* The aforeſaid *Corol.* eaſily flows from this *Theorem*; ſince in a given time an equal maſs of blood runs thro' the *aorta* and capillary arteries, and the length of the tube, conſiſting of the *aorta* and capillary arteries, is greater than the *aorta* alone. This the Dr. has the rather obſerved, becauſe not only *Michelotti*, but other mathematical writers in treating of forces, which either put into motion the liquor, contained in pipes, filled therewith, or ſtop its efflux, conſider only the maſs and velocity of the effluent fluid, without regarding the length of the pipes: For, *cæteris paribus*, a fluid is with greater difficulty either thrown out of a full pipe, or ſtopped in its efflux, the longer the pipe is; ſince the whole maſs of fluid, contained in the pipe, muſt be put into motion, before any part thereof can flow out at the orifice; as alſo the entire maſs be neceſſarily ſtopped, to hinder the efflux of any part thereof juſt ready for it.

'The principles of *Bernouilli*'s demonſtration of the velocity of water running out of a hole of a full veſſel, are, that the loweſt drop of the liquor, or that immediately incumbent on the hole of the veſſel, is conſidered, as preſſed upon, or (as he calls it) animated by a certain accelerating gravity, which is to the natural gravity, as the height of the water, or of the whole liquor, incumbent on the hole of the veſſel, to the height of the ſmall drop; that is, as the abſolute weight of the column of water, inſiſting on the hole of the veſſel, to the abſolute weight of the drop: For thus, nothing remains, but to find how great a velocity the drop, animated by that greater gravity, may acquire, when it falls thro' a line, equal to its height; that is, after it has got quite out of the hole: For it is preſſed upon by the whole column of water, conſequently, animated by the greater gravity, ſo long as any of the drop (which he ſuppoſes a ſmall ſolid column) remains above the hole.'

The weakneſs of this foundation appears thus; ſince *Bernouilli* makes uſe of nothing to animate, as he calls it, the loweſt drop with the aforeſaid accelerating gravity, but preſſion alone, or the weight of the column of water inſiſting on the hole: Let all the water, ſurrounding that column, be ſup-

poſed to be frozen, and the column to fall without any reſiſtance along the ſmooth ſurface of the ice; then as long as the hole is ſhut up, the ſmall drop next the hole will be preſſed upon by the whole weight of the incumbent column of water, in the manner *Bernouilli* ſuppoſes.

Now let the hole be opened, and a free exit be given the water. What will then be the conſequence? *Will the loweſt drop be urged or animated by the accelerating gravity, which is to the natural gravity, as the height of the whole water incumbent on the hole, to the height of the drop?* By no means; but it will be urged only by its own natural accelerating gravity: For, as ſoon as the loweſt drop begins to deſcend, tho' with an infinitely ſmall velocity, it will no longer be urged by the weight of the incumbent column of water: For, it is impoſſible for the column of water to preſs upon the ſubjacent drop without being hindered in its deſcent by that drop: But it is not hindered, becauſe it does not endeavour to deſcend with greater velocity than the loweſt drop tends downwards by its own force of gravity; but the column and drop deſcend equally, ſo that the drop will neither quit the column, nor receive any force or preſſion from it.

The Dr. therefore, thinks *Bernouilli*'s demonſtration falls to the ground: His miſtake ſeems to be owing to his not adverting to the difference, between a body, preſſed upon by an incumbent weight, when that weight is only urged by the natural accelerating force of gravity, and a body impelled, or animated (as he calls it) by the accelerating force of gravity, preternaturally encreaſed: In the latter caſe, the body will deſcend with a greater velocity, than what can be produced by the natural force of gravity, according to *Bernouilli* himſelf: But in the former caſe, however the body, preſſed upon, while it is at reſt, may be urged by the incumbent weight; yet as ſoon as it begins to deſcend, it will do ſo entirely with the ſame degree of velocity, as if it were preſſed upon by no incumbent weight at all.

To illuſtrate this by an example; ſuppoſe a ſolid column, conſiſting of 100 pieces of gold, laid upon each other, at reſt upon a table, and the loweſt piece preſſed upon by the weight of the reſt; now if a hole be made in the table, under the pieces, that the undermoſt may ſlip thro'; as ſoon as it begins to fall, it is immediately freed from the weight of the incumbent pieces; and then the undermoſt piece and all the reſt will deſcend with the ſame velocity, as if there were only that loweſt piece upon the table.

If

If from the velocity, with which, according to *Bernouilli*, water runs out at a hole, and from the mass of water (as determined by that velocity) running out in any given time, any one would determine its motion, he would find it twice greater than what could be produced by the force of gravity, from the weight of the column of water, incumbent on the hole.

The Dr. recommends the two following experiments (in order with more certainty to determine the controversy) either to be tried a-new, or at least diligently considered: The one is Sir *Isaac Newton*'s described p. 305. *Princip. second. edit. viz.* From the mass of water, running out in a given time, to find the velocity with which it passes thro' the hole: The other is *Mariotte*'s in his book *de mouvement des eaux part* 2. *disc.* 3. *regl.* 1. and made with a cylindrical pipe, open at both ends, its lower part reflected upwards and full of water: Whence it may be easily estimated, whether the first drops of effluent water can rise to so great a height, as *Bernouilli*'s demonstration requires.

A remarkable instance of the Infection of the Small-pox; *by the Same.* Phil. Trans. N° 373. p. 191.

A Young gentleman, ill of that sort of small-pox, called the coherent, or intermediate species between the distinct and confluent kind, on *Wednesday* the 3. of *October* 1722, being the 6. day from the eruption, grew delirious in the night, and got out of bed in spite of two nurses that attended him; and seising one of them by the neck between his bare arms, he pressed her forehead against his naked breast, then covered with the small-pox, in the state of maturation, and held her for some time in that posture: She was heated by striving with him and in struggling to get loose, she was sensible, that she bruised and broke some of the pustules with her forehead: This woman was about 40 years of age, of a clear, florid, sanguine complexion: She told the Dr. she had had the small-pox, when about 7 or 8 years of age, and had been pretty full of them, tho' there were no marks on her face: On *Friday* morning the small-pox began to appear upon her forehead, and increased by degrees to between 50 or 60: She had likewise a few pustules on the back part and sides of her neck, where the patient had grasped her with his naked arms; but had none, as she told the Dr. on any other part of her body: The lower part of her face was entirely clear of them, and those upon her forehead were chiefly confined to the middle and most prominent part of it, that had

been

been preſs'd againſt the patient's breaſt: They roſe gradually, and came to maturity, in the ſame manner as the ſmall-pox of the milder coherent kind, with a great inflammation and ſwelling of her forehead, and the adjoining part of her face; eſpecially, between the eye-brows, where a ſmall cluſter of the puſtules were ſeated; inſomuch, that on the 9. of *October* her right eye was quite cloſed up, and the left almoſt in the ſame condition: But all this time ſhe had no fever, ſickneſs or other ſymptom of the ſmall-pox, beſides this eruption, and the inflammation and pain that attended it. That night ſhe cauſed a bliſter to be applied to her neck; upon which ſhe recovered the ſight of her eye the next day, being the 6. from the eruption, when the puſtules were turning and beginning to ſcab: The ſcabs agreed with thoſe of the milder coherent ſort in their appearance and duration. The Dr. ſaw her ſeveral times after this; particularly on *Monday* the 22. of *October*, which was the 18 day from the eruption of the puſtules, when ſhe had ſtill ſome ſmall part of the ſcabs, remaining on her forehead.

In this inſtance it is worthy remark, 1. That this woman, tho' ſhe had had the ſmall pox before, was notwithſtanding infected again by the immediate and cloſe application of the variolous matter to her ſkin, when her body was heated with exerciſe: Which ſeems to prove, that ſuch an application is more effectual to give the infection than the bare morbid effluvia, ariſing from the body of the patient, and received into the ſound body by inſpiration: For, that ſhe received no infection by inſpiration is plain, from the appearing of the ſmall-pox upon thoſe parts only, where there had been ſuch an application and contact: From which it appears very probable, that a perſon who has already had the ſmall-pox, as the man, inoculated by Mr. *Tanner* in St. *Thomas*'s hoſpital, may poſſibly receive it again in ſome ſlight degree by inoculation; that being ſtill a more cloſe and immediate application of the variolous matter to the blood, and juices of the ſound perſon, than when it is applied only by contact to the ſkin, whole and unwounded.

2. That the infection, communicated to this woman, not being univerſal, as appears from her having no fever or ſickneſs, or general eruption of the puſtules all over her body, but only on the parts, infected by immediate contact; no argument can hence be drawn, for a perſon's being liable to undergo the ſmall-pox a ſecond time; ſo as to have the uſual ſymptoms of that diſeaſe, and a general eruption of the puſtules; but rather the contrary.

3. That

3. That the time, in which this infection ſhewed itſelf, by the appearance of the puſtules, is very different from that obſerved upon inoculation; the firſt appearing in about a day and a half; whereas in the latter caſe, the eruption generally ſhews itſelf on the 10. day or not above a day ſooner or later, as appears from the accurate and curious obſervations of Dr. *Nettleton*. Which difference is what ought in reaſon to be expected; ſince in one caſe the infection went no farther than the parts affected by immediate contact; whereas, in the other it muſt be propagated thro' the maſs of blood to all the parts of the body.

An Account of two Obſervations upon Cataracts; *by* S. Antonio Benevoli. Phil. Tranſ. N° 373. p. 194.

ON the 13. of *July* 1720, S. *Benevoli* couched a *German* ſoldier of cataracts in both his eyes, who immediately after the operation recovered the ſight of them and continued to ſee till his death, which happened of an acute illneſs on the 6. of *April* 1722. Upon this S. *Benevoli* took the eyes out of their orbits, in order to examine, whether the cataracts, conſiſted of a membranous pellicle, as ſome writers affirm; or of a preternatural opacity in the chryſtalline humour, as others pretend. Proceeding immediately to the diſſection of the left eye, upon a careful and very exact examination of all its contents, he could not find any ſuch thing as a pellicle within it; but diſcovered a ſmall yellowiſh body at the bottom of the bulb of the eye, of a lenticular ſhape, without adhering to any of the parts of the eye; which, upon extracting it, appeared to be the chryſtalline humour grown opaque and ſomething leſs than its natural ſize, having two or three ſmall dents or impreſſions, made in its circumference, which it had received from the needle, in the operation of couching.

The next day he examined the right eye in the ſame manner, and he diſcovered therein the chryſtalline humour grown opaque and depreſſed, in the ſame manner as the former, to the bottom of the eye; ſtill evidently retaining the marks of the needle; but he found no pellicle within the eye, notwithſtanding the ſtricteſt ſearch he could make.

S. *Benevoli*, having formerly made experiments on the eyes of dead ſubjects, at *Bologna*, in company with Dr. *Valſalva*, introduced the needle into the eye in the ſame place and in the ſame manner, as is commonly practiſed in the operation of couching; and having afterwards diſſected the ſame eyes, he always found, that the needle had paſſed into the eye on the back-

backside of the chryſtalline humour; ſo that it was impoſſible to bring the needle forwards from thence into that part of the aqueous humour, ſeated between the *uvea* and chryſtalline humour, in order to depreſs a pellicle, ſeated there, according to the common opinion, unleſs he had paſſed his needle thro' the ſubſtance of the chryſtalline humour.

This curious author likewiſe obſerves, that the aforeſaid ſpace, between the *uvea* and chryſtalline humour, is ſo very narrow, that tho' he finds it not impoſſible to introduce a needle into that ſpace; yet there is by no means room enough to turn the needle up and down in all directions, with that freedom, uſed in couching cataracts, without wounding either the *uvea* or chryſtalline humour.

Laſtly, S. *Benevoli* obſerves, that in his practice of couching cataracts for ſeveral years (having generally couched about 12 or 14 in a year) he had always found, that he worked upon a hard and reſiſting ſubſtance, which being tenderly touched by the needle, would vibrate and fluctuate backwards and forwards, and ſometimes return againſt the needle with a ſenſible *impetus*, which by no means agrees with the common notion of the cataracts conſiſting in a pellicle or membranous ſubſtance.

An Obſervation of a ſolar Eclipſe *at* Greenwich *Nov.* 27. 1722. *p. m. by Dr.* Halley. Phil. Tranſ. N° 374. p. 197. *Tranſlated from the* Latin.

h	′	″	
1	29	16	DR. *Halley* obſerved the eclipſe juſt begun.
1	31	6	The diſtance of the cuſps 7′ 4″; whence the part eclipſed 47″ and the true beginning was at 1^h 28′ 58″.
1	34	18	The diſtance of the cuſps 10′ 50″.
1	42	28	Repeated 16′ 20″.
1	43	26	The inclination of the cuſps to the right 44° 30′.
2	32	37	The remaining clear parts 17′ 20″.
2	40	16	Again repeated in the middle of the eclipſe nearly, 17′ 9″.
3	28	45	The inclination to the left 19°.
3	31	45	The diſtance of the cuſps 15′ 21″.
3	37	35	Again repeated 10′ 50″.
3	43	25	The end of the eclipſe doubtful, by reaſon the ſun's limb was uneven and undulating and not well defined.

h	′	″	
3	43	45	The eclipſe certainly ended.

The ſky was all the time ſerene and calm.

The ſame Eclipſe *obſerved in* Fleetſtreet, London; *by Mr.* George Graham. Phil. Tranſ. N° 374. p. 198.

	h	′	″	
P. M.	1	28	38	Apparent time, the eclipſe began.
	2	29	34	By eſtimation, the cuſps parallel to the horizon.
	3	43	22	The end.
	2	14	44	The duration.

The quantity eclipſed five digits $\frac{716}{1000}$.

Mr. *Graham* had very correct obſervations both of the ſun and ſtars, the 26, 27, and 28. *Nov.* for determining the exact time by his clock.

For ſome minutes before the eclipſe began, he obſerved the ſun with a 12 foot teleſcope, furniſhed with a micrometer, keeping that part of the limb in the middle of the glaſs, where he expected the moon firſt to touch; and in leſs than four ſeconds of time, from the moment he judged the eclipſe began, it was ſo conſiderably advanced, that he could not doubt of having the beginning to leſs than three ſeconds: He believes the exact time of ending was within the ſame limit, notwithſtanding that the undulation of the limb was then much greater than at the beginning: At the time of the greateſt obſcuration, the parts eclipſed, meaſured with the micromer, were 927 ſuch parts, as the ſun's vertical diameter contained 1946, which was taken a little before the beginning of the eclipſe.

The ſky was clear and free from clouds, till near the end, when a narrow cloud obſcured ſome part of the ſun's diſk; but that part of the limb, where the eclipſe ended, continued clear till after it was over.

By this obſervation the beginning did not differ 2′ and ½ and the end not ½ a minute from Dr. *Halley*'s computation; and if the Dr's computation, which was made for *Greenwich*, had been reduced to the meridian of *London*, the difference would have been ſtill leſs.

The ſame eclipſe was obſerved by Mr. *Hawkins* at *Wakefield* in *Yorkſhire*, to begin at 1^h 21′ *p. m.* and to end at 3^h 30′ 3″. The ſun's diameter was obſcured upwards of five digits.

An Account of the Particles and Structure of Diamonds; *by M.* Leewenhoeck. Phil. Tranſ. N° 374. p. 1959 *Tranſlated from the* Latin.

AFTER M. *Leewenhoeck* had diſcover'd, that ſome metals, and even grains of ſand, conſiſted of very ſmall particles of the ſame matter, he turn'd his thoughts to conſider a diamond, as whether it alſo conſiſted of the ſame ſort of particles, that might be obſerv'd with the microſcope.

M. *Leewenhoeck*, therefore, viewing with a microſcope a ſmall diamond, could obſerve with the naked eye a great many particles in the unpoliſh'd and dark part thereof; and he found, that it conſiſted of ſmall particles: But not being ſatisfied with theſe obſervations, he reſolv'd to break a diamond into pieces, in order to conſider theſe pieces.

Laying, therefore, a diamond upon a hammer, he ſtruck it once and again with another hammer, and thereby it was broke into four or five pieces: With which not being yet ſatisfied, he had a mind to break the diamond into ſtill ſmaller bits, and accordingly he folded up a piece, bigger than the reſt in a double piece of paper, that he might loſe none of it.

Here M. *Leewenhoeck* was ſurpris'd at the hardneſs of a diamond; which tho' often ſtruck upon with conſiderable force, broke only into four or five pieces without any ſmall bits.

Upon placing the laſt mentioned pieces before a microſcope, he view'd them, and found that all of them conſiſted of very ſmall particles: And upon expoſing them to the rays of the ſun, he obſerv'd a kind of little flame break out from them, and bigger than any he had ever obſerv'd.

He obſerv'd with his naked eye one ſmall piece that had its flat and ſquare crack, directly expos'd to the ſun; and as far as he could judge with his naked eye, three or four hairs of a man's beard in breadth.

Such a quantity of ſparkling flames iſſued from this piece of diamond, that he judged them upwards of 400: A few of thoſe flames lay cloſer to each other and were bigger than the reſt: Whence he concluded, that the particles of the diamond were bigger in that place and more regularly diſpos'd than the other particles.

After

PLATE VII.

Fig. I.

Fig. II.

Fig. III.

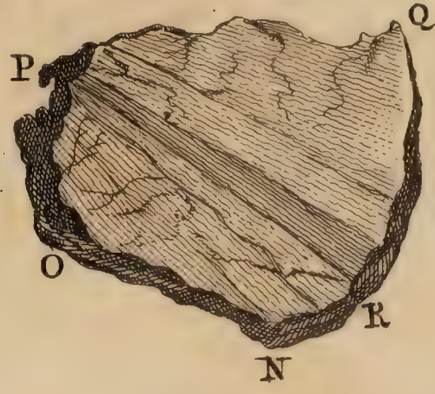


Fig. VI.

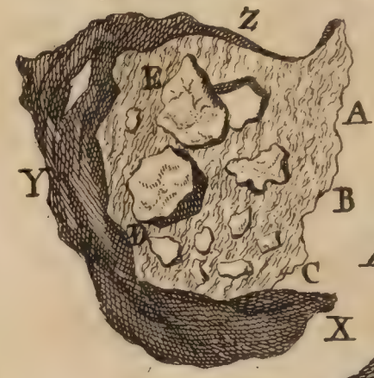
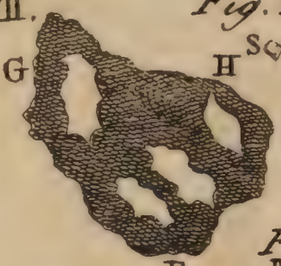


Fig. VII.



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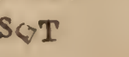


Fig. V.

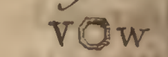


Fig. X.

Fig. XI.

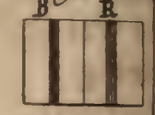


Fig. VIII.

Fig. IX.

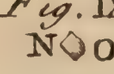


Fig. XIII.

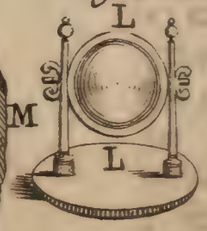


Fig. XV.

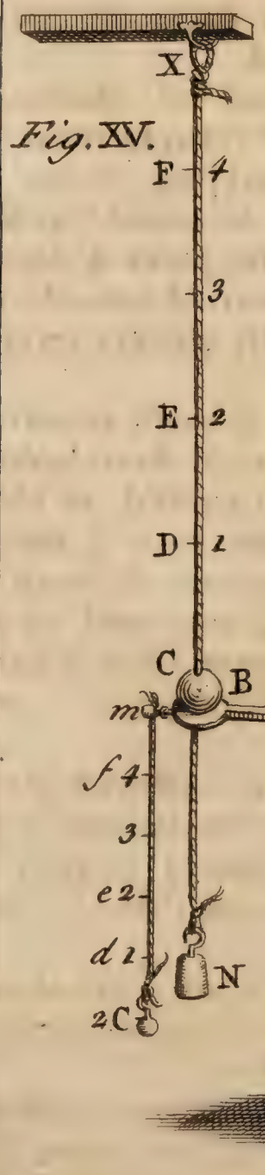


Fig. XII.

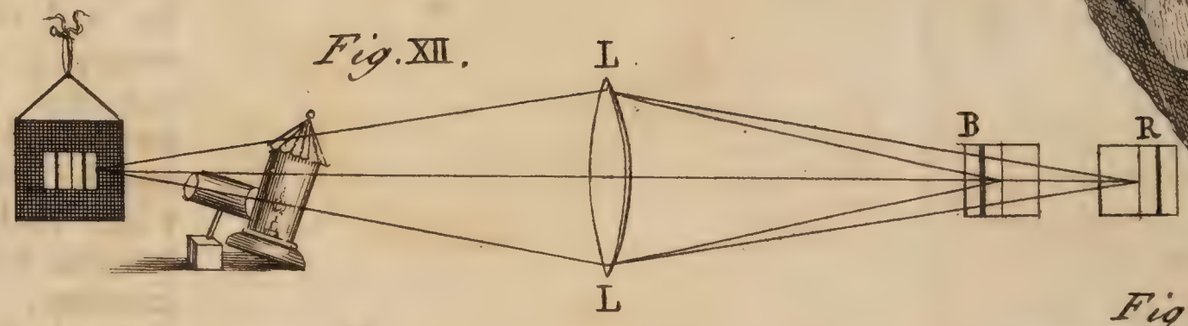
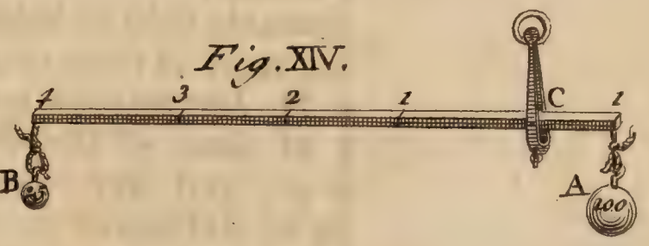


Fig. XIV.



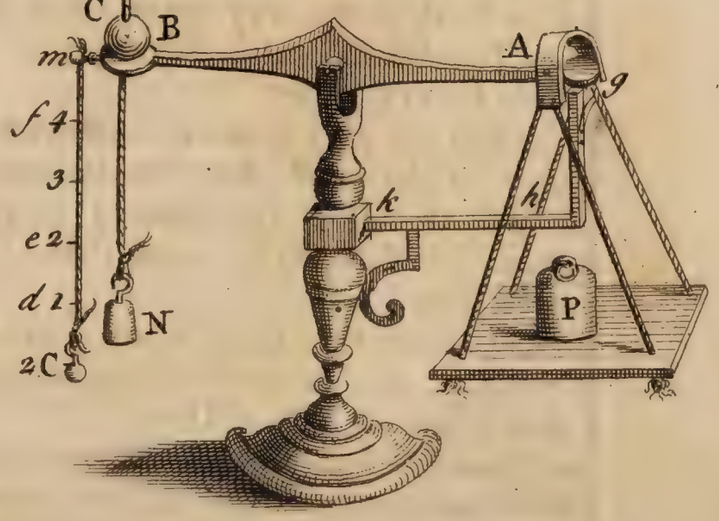


Fig. XVII.

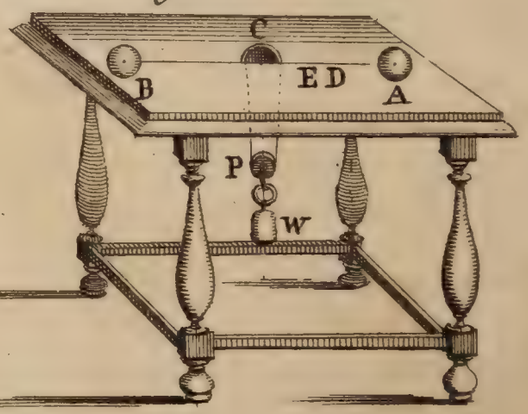


Fig. XVIII.

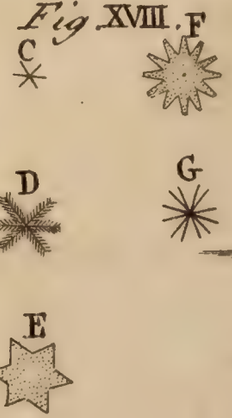


Fig. XVI.

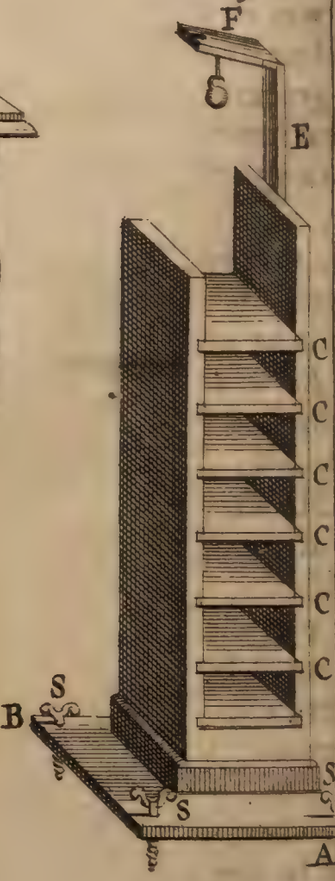


Fig. XIX.

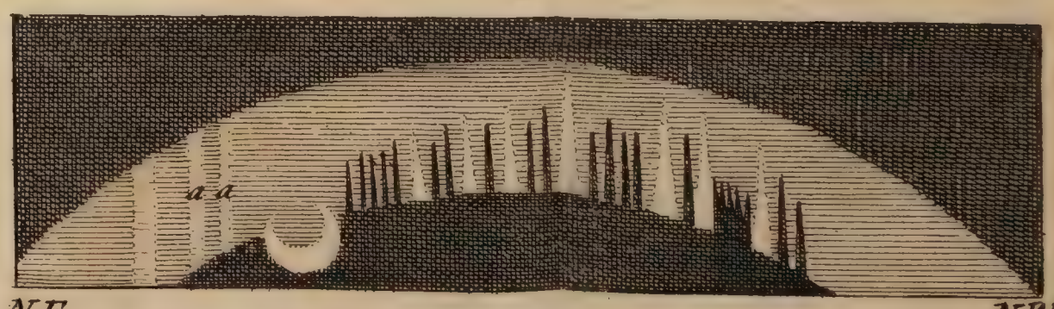


Fig. XX.

G. Smith del.

Hulett Sculp

Afterwards he view'd another piece of diamond, of about the ſame bigneſs with the former, which was alſo directly expos'd to the rays of the ſun; and he found it conſiſted of the ſame number of very ſmall particles: From the half of that little piece, there likewiſe aroſe the ſame ſparkling flames, but ſmaller; and in the other half a kind of waving flame was obſerv'd, with a continual coruſcation, like a faint lightning.

Moreover, after withdrawing theſe little pieces out of the ſun's rays, various appearances ſtill preſented themſelves to his eyes; among other things a little flame ſeemed to dart aloft from each particle of the diamond.

Moreover, he had nine ſmall pieces of diamond lying before his microſcope; and in ſeven of them he obſerv'd theſe particles, which ſhot forth the ſparkling flames as was already mentioned; and in two others he could likewiſe obſerve thoſe particles, of which he ſuppoſes a diamond is compos'd; but they had their planes turn'd towards the ſun, in ſuch a manner that he could diſtinctly obſerve ſeveral particles at the ſame time.

It was an agreeable ſight to behold ſo many appearances of ſparkling flames, moſt of them of a bright flame-colour, and ſome greeniſh: M. *Leewenhoeck* was ſurpris'd to obſerve at the extremity of ſome of the flames ſuch a motion and vibration in the air, as if they were become ſo weak in that place, as not to be diſtinguiſh'd. He was moſt of all ſurpris'd, that fire ſhot forth every way out of ſuch a particle of diamond, flaſhing faintly and like lightning at a diſtance; and this he obſerv'd ſeveral times.

M. *Leewenhoeck* did not only himſelf view with his microſcops the ſaid piece of diamond, but he ſhew'd it another perſon, who affirm'd, that the appearances exactly agreed with M. *Leewenhoeck*'s deſcription, and that he was ſurpris'd at the unuſual ſight.

Moreover, he had before his microſcope a piece of diamond whoſe particles were in *lamellæ* or layers on each other; and repreſented by ABC Fig. 1. Plate VII.

Afterwards he placed before his microſcope another piece of diamond, whoſe *lamellæ* he could obſerve very diſtinctly lie on each other; and repreſented by DEFGHIKLM Fig. 2. where the particles mark'd FG, FH, FI, FK and L ſeem to be ſomewhat thicker than the reſt; but theſe conſiſt of ſeveral *lamellæ* lying upon each other. In that part of the

piece, repreſented between DEFM, exceeding thin *lamellæ* appear very diſtinctly from a congeries of which it is certain the whole diamond is compos'd.

M. *Leewenhoeck* had ſent him two pieces of diamond; as alſo a ſmall diamond, that was cut or poliſhed, but foul or flawy, as jewellers term it.

The ſmall lines running thro' all the piece NOPQ Fig. 3, are really no other than the *lamellæ*, of which diamonds are ſaid to be compos'd; which are more diſtinctly ſeen at PQ.

In order to have a more diſtinct idea of this he caus'd the deſigner to draw it of the ſame bigneſs, as it appear'd to the naked eye; and this is repreſented between ST Fig. 4. This ſmall piece of diamond conſiſted of ſo many and ſuch ſmall particles, as cannot be conceiv'd by any, who have not ſeen it.

In another piece of diamond, almoſt of the ſame bigneſs, the *lamellæ* could be diſtinguiſh'd; and about $\frac{1}{5}$ part thereof conſiſted of ſo regular a *pentagon*, as if it were artificially cut, only that a very ſmall diamond was faſtened to it, that cover'd about the fourth part of the *pentagon*; and he could plainly ſee, that it alſo conſiſted of *lamellæ* or particles reſembling *lamellæ*.

As to the ſmall diamond, tho' poliſhed, it was foul or flawy; ſuch being ſo as are either of a yellowiſh colour or full of lines or chinks, tho' thoſe in this could not be obſerv'd with the naked eye, nay, ſcarcely with the microſcope.

This ſmall diamond was quadrangular; near one of the angles within the diamond, M. *Leewenhoeck* obſerv'd various particles, ſeparated from each other, only that they ſeem'd to touch each other a little, at which he was at firſt ſurpris'd; the largeſt of theſe particles was of a yellowiſh colour, and broader than the other, but not ſo thick, and as bright as glaſs; the other particles were of various figures, and ſome as bright as the firſt; and M. *Leewenhoeck* judged them about 20 in number, tho' the deſigner had not delineated ſo many.

Upon viewing this, M. *Leewenhoeck* was apt to think, that when the matter, of which diamonds are form'd, floated in the air; theſe ſmall particles, which likewiſe were diamonds, were united to the former diamond.

Upon this occaſion M. *Leewenhoeck* recollected, that he had ſeveral mountain chryſtals of an hexangular figure, within ſome of which lay ſome very ſmall oblong figures, of

a

a blueiſh colour; but ſo exceeding ſmall, as hardly to be diſtinguiſh'd, even in the beſt microſcope.

Moreover, he caus'd delineate the flawy diamond, as big as the life, as repreſented between V and W Fig. 5.

Afterwards he caus'd delineate ſeparately thoſe ſmall diamonds, included in the larger one, repreſented in Fig. 5. and ſhewn at X Y Z A B C Fig. 6. where X Y Z denotes the outer ſide of the diamond, which tho' cut or poliſhed, yet view'd thro' a microſcope, appear'd of a darkiſh colour; Z A B C D E repreſents thoſe ſmall diamonds, which lay inclos'd, as it were, in the larger diamond.

Afterwards placing before his microſcope that ſide of the diamond, repreſented Fig. 5. and in which the ſmaller diamonds were inclos'd, as has been ſaid, he found it full of holes, which M. *Leewenhoeck* ſuppoſes, were made in poliſhing that ſide: So that the ſmall diamonds, which poſſeſſed theſe places, falling out in the poliſhing, caus'd theſe pits or holes, repreſented between F G H Fig. 7.

Moreover, turning that ſmall diamond (repreſented between V W Fig. 5.) upon its ſide; and where it was thickeſt, applying a razor, in order to ſplit it with the blow of a hammer; but after repeated blows he could not effect it.

Wherefore, folding the diamond up in a clean piece of paper, he laid it upon a hammer, and after ſtriking ſeveral blows with another hammer, he at laſt broke it. After he had placed all the broken pieces before different microſcopes, he caus'd the deſigner delineate one piece, that ſeemed to the eye to have more *lamellæ* than the reſt, as repreſented by I K L M Fig. 8. Yet it was not poſſible for the deſigner to draw it ſo compleatly as it appear'd to the eye.

The microſcope, with which that piece, repreſented Fig. 8. was obſerv'd, being different from the other microſcopes, by which the other pieces of diamond were delineated; the deſigner drew this laſt piece of the ſame bigneſs, as it appear'd to the naked eye, as repreſented between N O Fig. 9.

Some of the pieces of the diamond, view'd with a microſcope, exhibited a very agreeable ſight; and M. *Leewenhoeck* ſhew'd them to others, who were mightily pleas'd to obſerve ſuch a variety of parts in one ſingle piece of diamond; eſpecially that the *lamellæ*, of which diamonds are compos'd,

could

could be very distinctly observ'd in two small pieces; to wit when these *lamellæ* lay lengthways before the eye.

Afterwards M. *Leewenhoeck* turn'd his thoughts to examining an hexagonal piece of mountain chrystal, whose length was about two fingers breadth, and its thickness that of the little finger.

He broke that chrystal into several pieces, which he placed before his microscopes, in order to examine, whether these were compos'd of *lamellæ*, laid upon each other, in the same manner as he said, diamonds acquire their bulk: But after repeated trials, he could not find even the least *lamellæ* therein: But he generally found, in the six sides of all the pieces of chrystal he had, small transverse lines, some a little higher than the others.

Notwithstanding M. *Leewenhoeck* had formerly considered and broke a great many chrystals, yet he could never satisfy himself in this matter.

The different Refrangibility of coloured Light; *by Dr.* Desaguliers. Phil. Trans. N° 374. p. 206.

SIR *Isaac Newton* in his Optics, *book* 1. *prop.* 1. *exper.* 2. gives an account of an experiment, made with a card or paper, painted red on one half and blue on the other; which being enlightened by a candle, the image, by the interposition of a *lens*, is so projected on a white paper, held on the other side of the *lens*, that the place where the blue half appears distinct, (or the distinct base of the image of the blue half, as opticians call it) is much nearer to the *lens* than the place of the image of the red half; and this is made apparent, by seeing on one of these images the representation of the black threads, wrapp'd round the card, whilst they are not visible on the other: This is fully describ'd in *loc. supra citat.* But yet a foreign gentleman (vide *Acta Erudit. Lips. supplem. tom.* 8. § 3. *p.* 130, 131.) has call'd the experiment in question, and denied the matter of fact, affirming, that he could not make it succeed; but proposes an experiment of his own to disprove the different refrangibility of the rays,

Upon this Dr. *Desaguliers* made the experiment over again before the Royal Society, which succeeded well: But because there must be care taken in making it, he mentions all the particulars, observ'd in the performance; which, if duly

duly put in practice, will make the experiment always ſucceed.

He painted one half of the card R B (in Fig. 10. Plate VII.) as B with *ultramarine*, made deeper with a ſmall mixture of indigo; and the other half R he painted over with *cinnaber*, heighten'd with a little *carmine*; ſo that the line, which ſeparated the red from the blue, was perpendicular to the long ſides of the card.

Then he wrapp'd a black ſilk four times over the middle of each painted part of the card, as in Fig. 11.

Upon a ſquare trencher (as in Fig. 12.) painted black, and ſuſpended vertically againſt a wall, the Dr. fix'd the card with a pin; and the room being made very dark, he enlightened the card with a ſtrong light, thrown upon it from a dark lanthorn, that had two convex glaſſes therein; then ſetting up the *lens* L L (repreſented by Fig. 13.) in ſuch a manner that its axis paſs'd perpendicularly thro' the image of the card, at the diſtance of nine foot from the card, the image of the card being receiv'd upon a piece of white paper at the diſtance of nine foot on the other ſide of the *lens* at B, the blue half appeared diſtinct, with the image of the black ſilk going vertically along its plane, whilſt no appearance of the black ſilk was perceivable on the red half; then removing the paper about two inches to R, the red half of the image had a black line very plain upon it, whilſt it was inviſible on the blue half. This was more evident, when a ſtrong image of the candle was ſucceſſively thrown on that half of the card, whoſe image was then under conſideration: When the paper was held in the middle between R and B, the black line upon each colour was viſible, but indiſtinct.

N. B. Care muſt be taken to make the colours deep, becauſe having accidentally rubb'd off ſome of the blue, the whiteneſs of the card under it, caus'd its image to fly out farther, almoſt as far as that of the red.

An Account of the Inoculation of the Small-Pox *and the Mortality of that Diſtemper in the natural Way; by Dr.* Nettleton. Phil. Tranſ. N° 374. p. 209.

THERE are two propoſitions, advanced by the favourers of the practice of inoculation, in which the public ſeems to require fuller ſatisfaction. 1. That the diſtemper, rais'd by inoculation, is really the ſmall-pox. 2. That it

is much more mild and favourable, and far leſs mortal than the natural ſort.

The former of theſe is not ſo much diſputed now, as it was at firſt, when this method was introduced; nor can it be doubted by any one, who has ſeen thoſe that have been inoculated, and has alſo been much converſant in the natural ſmall-pox: There is uſually no manner of difference to be obſerv'd between the one ſort and the other, when the number of puſtules is nearly the ſame; but in both there are almoſt infinite degrees of the diſtemper, according to the diverſity of that number. All the variation, that can be perceiv'd in the inoculated ſmall-pox from the natural, is, that in the former, the puſtules are commonly fewer in number, and all the reſt of the ſymptoms in the ſame proportion more favourable: They exactly reſemble what is call'd the diſtinct ſort; the ſymptoms before the eruption are the very ſame, and when the puſtules begin to riſe, their appearance is the ſame, as well as their periods of maturation and declenſion; they are at firſt of the ſame florid, roſy colour, and when fully ripe, of as fair a yellow; they commonly riſe as round and as large as the other, and when they are very numerous, the inflammation and ſwelling of the face comes on at the uſual time, and is followed by the ſwelling of the hands and feet, and only once the Dr. obſerv'd a ſalivation, tho' the puſtules were diſtinct: In the natural ſmall-pox, when the puſtules are very few, we ſometimes obſerve, they do not riſe to ſo great a bulk, neither do they ripen ſo fully nor continue ſo long as uſual; and it is the ſame in the way of inoculation: In ſhort, as this diſtemper is rais'd by an ingrafiment from the ſmall pox, as it has the very ſame appearance, and as it is capable of producing the ſame by infection, there ſeems to be no room to doubt of its being the true and genuine ſmall-pox: And if that be allow'd, it will follow from thence, as a corollary, *viz.* That thoſe, who have been inoculated, are in no more danger of receiving the diſtemper again, than thoſe who have had it in the ordinary way: And this is alſo thus far confirm'd by experience. It is very readily granted, that the operation may ſometimes fail: Thoſe gentlemen, who firſt communicated to the Royal Society ſome account of this practice from *Turkey*, intimated ſo much; tho' Dr. *Nettleton* believes that that will happen but rarely: In one inſtance in *Halliſax*, vide *Phil. Tranſ.* N° 370, the Dr. obſerv'd no eruption at all, nor did

the

the wound inflame and ſwell any more than in a common inciſion, which made him conclude, that what was applied had not taken effect, and indeed, he was well aware of the reaſon. In three other inſtances, tho' the wounds inflamed, ſwelled and diſcharged conſiderably; yet the eruptions were ſo imperfect, as to leave the Dr. a little in doubt: But two of theſe were afterwards ſufficiently tried, by being conſtantly with thoſe who had the ſmall-pox, without receiving any infection; which makes the Dr. apt to think, they will always be ſecure from any danger of it: As to all the reſt, neither the Dr. nor any body elſe, who ſaw them, did in the leaſt queſtion, but that they had the true ſmall-pox.

As to the latter propoſition, *viz.* That the inoculated ſmall-pox is far leſs dangerous than the natural, the truth hereof, the Dr. ſuppoſes, can only be found by making a compariſon, ſo far as experience will extend. To this purpoſe he took an account how many had the ſmall-pox and how many out of that number died in the town of *Halifax* and ſome part of the country, and he procured the ſame from ſeveral other towns hereabouts, where the ſmall-pox had been epidemical in 1722, and that with as much exactneſs as was poſſible.

	Perſons who had the ſmall-pox.	died
in *Halifax*	276	43
in a part of the pariſh at *Halifax* ſtretching towards *Bradford.*	297	59
in another part of the ſame pariſh	268	28
in *Bradford*	129	36
in *Leeds*	792	189
in *Wakefield*	418	57
in *Rochdale*	177	38
in *Aſhton under Line*, a ſmall market-town in *Lancaſhire*, including two neighbouring villages.	279	56
in *Macclesfield*	302	37
in *Stockport*	287	73
in *Hatherfield*	180	20
Total	3405	636

A great number of obſervations is requiſite, before any certain concluſions can be drawn. The Dr. only remarks, that it

appears from these accounts, that in 1722, in this part of the kingdom, almost 19 out of every 100, or near one fifth of those, who had the natural small-pox, died; whereas out of 6 inoculated, not one died.

A Comparison between the Danger of the Small-pox *in the Natural Way and that given by Inoculation; by Dr.* Jurin. Phil. Trans. N° 374. p. 213.

THE number of persons, who had the small-pox by inoculation in *England*, is, by the best information the Dr could procure, as follows;

Inoculated by Dr. *Nettleton*	66
Mr. *Claudius Amyand* serjeant-surgeon	11
Mr. *Maitland* surgeon	55
Dr. *Dover*	
Mr. *Weymish* surgeon	
The Rev. Mr. *Johnson*	
Dr. *Brady* at *Portsmouth*	
Mr. *Smith* surgeon and Mr. *Dymer* apothecary at *Chichester*	11
Mr. *Waller* apothecary at *Gosport*	
A woman at *Leicester*	
Dr. *Williams* at *Haverford-West*	
Two other persons near the same place	
Dr. *French* at *Bristol*	
In all	183

The opposers of inoculation affirm, that out of this number two persons died of the inoculated small-pox; and the favourers of this practice maintain, that their death was owing to other causes. If, to avoid dispute, these two be allowed to have died of inoculation, we must estimate the hazard of dying of the inoculated small-pox, as far as can be collected from our own experience, to be that of 2 out of 182 or 1 out of 91.

Mr. *Mather*, in a letter, dated *March* 10. 1721, from *Boston* in *New England*, gives an account, that of near 300 inoculated there, five or six died upon or after it; but from other diseases and accidents, chiefly from having taken the infection in the common way by inspiration, before it could be communicated to them in this way of transplantation.

If we allow 5 out of these 300 to have died of the small-pox by inoculation, notwithstanding what Mr. *Mather* has said of their dying by other accidents or diseases; the hazard

of inoculation will thence be determined to be that of 1 in about 60: But here it muſt be obſerved, that by all the accounts from *New England*, the operators there do not appear to have been ſo cautious in the choice of their ſubjects, as in *England*: For, Mr. *Mather* tells us, that the perſons inoculated were young and old, from 1 to 70 years of age, weak and ſtrong; and by other relations we are informed, that women with child, and others even in child bed, underwent the operation: Probably the greatneſs of the danger they were in, from the infection in the natural way, which then raged among them with the utmoſt fury, made them the more adventurous.

In order to form an eſtimate of the hazard, which all mankind, one with another, are under of dying of the ſmall-pox in the natural way, that by comparing this with the hazard of inoculation, the public may be enabled to form a judgment, whether or no the practice of inoculation tend to the preſervation of mankind, by leſſening the danger to which they are otherways liable; the Dr. conſulted the yearly bills of mortality, as far back as 1667, the year after the plague and the fire of *London*, comprehending to 1722, the ſpace of 56 years, from 42 of which the Dr. has given extracts in the two following tables.

The firſt of theſe takes in the firſt 20 years, diſtinguiſhing for every year the total number of burials, and likewiſe the number that died of the ſmall-pox in two ſeparate columns. The third column ſhews, how many died of the ſmall-pox out of every 1000 that were buried; and the fourth column exhibits the proportion between thoſe that died of the ſmall-pox and the whole number of burials, by the neareſt vulgar fraction, having always 1 for the numerator.

The ſecond table gives the laſt 22 years after the ſame manner; and at the bottom of each table is given the total number for each ſeries of years, and likewiſe the number that died each year, taken at a medium, one year with another: By which it appears, that the proportion between the number of thoſe that die of the ſmall-pox and the whole number of burials, is very nearly the ſame, upon an average for each ſeries of years.

The 14 intermediate years between 1686 and 1701 are left out; becauſe in the bills for thoſe years, the accounts of the ſmall-pox and meaſles are not diſtinguiſhed, as in the preceeding and following years; but are joined together in one article: So that from them no certain account can be drawn of the number of perſons that died of the ſmall-pox.

TABLE I.

Years	Total N° of Burials	Died of the Small Pox. In all	In 1000	In Propor.
1667	15842	1196	75	1/33
1668	17278	1987	115	1/9
1669	19432	951	49	1/20
1670	20198	1465	73	1/14
1671	15729	696	44	1/23
1672	18230	1116	61	1/16
1673	17504	853	49	1/21
1674	21201	2507	118	1/8
1675	17244	997	58	1/17
1676	18732	359	19	1/52
1677	19067	1678	88	1/11
1678	20678	1798	87	1/12
1679	21730	1967	91	1/11
1680	21053	689	33	1/31
1681	23971	2982	125	1/8
1682	20691	1408	68	1/15
1683	20587	2096	102	1/10
1684	23202	156	7	1/149
1685	23222	2496	107	1/9
1686	22609	1062	47	1/21
20 Years	398200	28459	71½	1¼
Each Y. at a *Medium*	19910	1423	71½	1/14

TABLE II.

Years	Total Nº of Burials.	Died of the Small Pox.		
		In all	In 1000	In Propor.
1701	20471	1095	53	1/19
1702	19481	311	16	1/63
1703	20720	898	43	1/22
1704	22684	1501	66	1/15
1705	22097	1095	50	1/20
1706	19847	721	36	1/28
1707	21600	1078	50	1/20
1708	21291	1687	79	1/13
1709	21800	1024	47	1/21
1710	24620	3138	127	1/8
1711	19833	915	46	1/22
1712	21198	1943	92	1/11
1713	21057	1614	77	1/13
1714	26569	2810	106	1/9
1715	22232	1057	48	1/21
1716	24436	2427	99	1/10
1717	23446	2211	94	1/11
1718	26523	1884	71	1/14
1719	28347	3229	114	1/9
1720	25454	1440	57	1/18
1721	26142	2375	91	1/11
1722	25750	2167	84	1/12
22 Years	505598	36620	72	1/14

Each

	Tot. Nº	Died of the Small Pox.		
	Bur.	In all	in 1000	in Pro.
Each Year at a *Medium.*	22982	1665	72	$\frac{1}{14}$
42 Ye.	903798	65079	72	$\frac{1}{14}$
Each Year in 42 at a *Medium*	21519	1550	72	$\frac{1}{14}$

By thoſe tables it appears, that upwards of 7 *per cent* or ſomewhat more than $\frac{1}{14}$ part of mankind die of the ſmall-pox; and conſequently, the hazard of dying of that diſtemper, to every individual, born into the world, is at leaſt that of 1 in 14; and that this hazard increaſes, after the birth, as the child advances in age, will appear from what follows.

From this eſtimate it is demonſtrable, that in the caſe of perſons actually having the ſmall-pox, the hazard they run, one with another, of dying of that diſtemper, is greater than that of 1 in 14; or which is the ſame thing, there muſt be fewer than 13 that recover, for one that dies of the ſmall-pox: For, ſince $\frac{1}{14}$ part of mankind die of the ſmall-pox and the other 13 parts die of other diſeaſes; if theſe 13 have all had the ſmall-pox and recovered from them, before they fell ill of thoſe other diſeaſes of which they died, then juſt 13 will have recovered from the ſmall-pox, for one that dies of that diſtemper: But as it is notorious, that great numbers, eſpecially of young children, die of other diſeaſes, without ever having the ſmall-pox; it is plain, that fewer than 13 muſt recover from this diſtemper, for one that dies of them.

To determine exactly how many of theſe 13 parts of mankind die, without having the ſmall-pox, is a very difficult taſk: But it is eaſy to ſee, that a conſiderable deduction is to be made from them.

In the firſt place, the two articles of ſtill-born and abortive children, which are put into the yearly bills, as part of the number of burials, are unqueſtionably to be deducted.

With theſe two the Dr. joins the following heads, which, by the beſt information he could procure, comprehend only very young children, or at moſt not above one or two years of age; as overlaid, chryſoms and infants, convulſions, horſeſhoehead, headmoldſhot, teeth, water in the head, worms, rickets, liver-

grown,

grown, chin-cough and hooping-cough; which articles in the yearly bills for 22 years laſt paſt, amount at a medium to 386 in each 1000, of the whole number of burials.

It is true, that in all probability, ſome ſmall part of theſe muſt have gone thro' the ſmall-pox; and therefore ought not to be deducted out of the account: But then, on the other hand, as it is certain, that of the remaining $\frac{614}{1000}$ of mankind, that are above one or two years of age, there are great numbers, that never have the ſmall-pox; it will be judged no unequal ſuppoſition, if we ſuppoſe all contained under the above-mention'd heads, to have eſcaped that diſtemper, when by way of compenſation all the reſt of mankind are allowed to undergo them, which conceſſion is ſo large, that it will abundantly make up for what is aſſumed too much in the former ſuppoſition.

Allowing, therefore, that out of every 1000 children, that are born, 386 die under 1 or 2 years of age, without having the ſmall-pox, and 72 do ſome time or other die of that diſtemper; it follows, that the hazard of dying of them to the remainder of mankind, above 1 or 2 years of age, who are all ſuppoſed to undergo that diſeaſe ſooner or later, is that of 72 out of 614, or nearly 2 out of 17: So that no more than between 7 and 8 can recover from that diſtemper, for one that dies of them: And if any conſiderable part of the aforeſaid remainder of mankind, more than is allowed for above, eſcape having the ſmall-pox, then the proportion of thoſe that recover from them, will be ſtill ſmaller.

This conſideration ſhews the fallacy of one plauſible argument, that has been often made uſe of on occaſion of the diſputes about inoculation; *viz.* that whatever be the danger of dying of the ſmall-pox, to thoſe that actually have that diſeaſe; yet as great numbers of perſons never have the ſmall-pox at all, this danger is what any particular perſon may never be in, and therefore, it will be madneſs to undergo the hazard of inoculation, be it great or ſmall, in order to prevent a diſeaſe, which poſſibly may never befal one.

For, if 2 parts in 17 of all mankind, that are above one or two years of age, muſt ſooner or later die of the ſmall-pox; it is plain, that how many parts ſoever of theſe 17 are ſuppoſed to eſcape that diſtemper, the mortality among the remainder who undergo them, muſt in proportion be ſo much the greater. As for inſtance, if 7 parts eſcape having the ſmall-pox and 10 under-

undergo them, then 2 out of 10, or 1 out of 5, that have the small-pox, muſt die of them.

And as it can never be known, whether or no any particular perſon be one of thoſe, that are to have the ſmall-pox; his hazard of dying of that diſtemper being made up of the hazard of having it, and the hazard of dying of it; if he have it, will be exactly the ſame, namely, that of 2 in 17, or 1 in 8 or 9, whether the proportion of mankind, that eſcape having the ſmall-pox, be great or ſmall.

But as what has been ſaid concerning the hazard of the ſmall-pox in the natural way, is taken from an account of 42 years: Whereas, the hazard of inoculation is eſtimated only from what has happened in the ſpace of about 18 months, ſince which time it had its firſt riſe among us; it will, perhaps, be aſked by ſome perſons, why the eſtimate of the hazard of the natural ſmall-pox is not alſo made, from the laſt two years alone, without running back into ſo great a number of years, before inoculation was begun?

To which the Dr. anſwers, that the proportion of ſuch, as die of the ſmall-pox, varies ſo much in different years, as appears from the tables above, that it was impoſſible to come at any certainty in this point, from the conſideration of the two laſt years alone; and if any one ſuſpect the Dr. of partiality in proceeding in the manner he has done, he need only caſt his eyes upon the ſecond table, where he will find, that the mortality of the natural ſmall-pox, for the 2 laſt years, has conſiderably exceeded the medium he has determined, from taking in 42 years.

There is another method, which, if it were put in practice in ſeveral large towns or pariſhes, and for a ſufficient number of years, would enable us to come at a nearer and ſtill more certain computation of the proportion between thoſe that recover, and thoſe that die of the ſmall-pox; which is, to ſend a careful perſon once a year, from houſe to houſe, to enquire what perſons have had the ſmall-pox, and how many have died of them in the preceeding year. This was done in ſome places, as follows.

Sick

	Sick of the small-pox	died
Several towns in *Yorkshire*	3405	636
Chichester	994	168
Haverford-West	227	52
Total	4626	856

From which it appears, that upon a medium between these accounts, there died of the small-pox almost 19 *per cent*, or nearly 1 in 5, of persons of all ages, that underwent that distemper. Which is the more to be remarked; for, that out of 82 persons, that had the small-pox by inoculation, the same year, and in the neighbourhood of the same places, not one miscarried.

Mr. *Mather* observes, in his abovementioned letter, that out of more than 5000 persons that had the small-pox at *Boston* in *New-England*, within little more than half a year, near 900 died, which is more than 1 in 6; and this account added to those from *Yorkshire*, *Chichester* and *Wales*, reduces the proportion of those that die of the small-pox to somewhat more than 18 *per cent*; so that the hazard of dying of that distemper, to those who are taken ill of it, is that of 1 in between 5 and 6, or something above 2 in 11.

The result, therefore, of these computations is, that if the same proportions should still continue, as have hitherto been determined by observation, we must expect.

That of all the children that are born, there will, some time or other, die of the small-pox, 1 in 14.

That of persons of all ages taken ill of the natural small-pox, there will die of that distemper, 1 in 5 or 6, or 2 in 11.

That of persons of all ages, inoculated, without regard to the healthiness or unhealthiness of the subject, as was practised in *New-England*, there will die 1 in 60.

That of persons, inoculated with the same caution in the choice of the subjects, as was used by the several operators one with another, in *England* (if we allow in the two disputed cases abovementioned, that the patients died of the inoculated small-pox) there will die 1 in 91.

But if those two patients be allowed to have died of other accidents or diseases; then we shall have reason to think, as far as any judgment can be made from our own experience in

England, that none at all will die of inoculation, provided that proper caution be uſed as in *Turkey*, where out of many thouſands, that in the ſpace of 40 years were inoculated in and about *Conſtantinople*, by one *Greek* woman, not ſo much as one perſon miſcarried, as the Dr. was aſſured by the ingenious Dr. *le Duc*, a native of *Conſtantinople*, who was himſelf inoculated there, under the care of his father, an eminent phyſician in that city.

The following account of the ſucceſs of inoculation in and about *Boſton* in *New England*, was procured from Capt. *Osborne*, who reſided in that town and neighbourhood, during the whole time of that practice.

The Dr. inſerts it here, as it confirms the extract given above from Mr. *Mather*'s relation, and is a more particular account of the matter of fact, than any he had hitherto ſeen.

In *May* 1721, the ſmall-pox were brought into the town of *Boſton*; in *June* they began to ſpread pretty much, and in *July* they were got into moſt parts of the town and a conſiderable number of people died of them: At this time inoculation was firſt put in practice by Dr. *Boyleſton*, who then performed it upon his own child, and a *Negro* ſervant, who both did well; notwithſtanding which, this attempt gave great uneaſineſs to the neighbours: However the practice went on, to the number of about 40 perſons, one of which was a woman of about 40 or 45 years of age, who got well over the ſmall-pox, as her huſband publickly declared, but had been before troubled with hyſteric fits, of which ſhe died ſome little time after: When about 70 perſons had undergone the operation, Capt. *Osborne* and his wife, who had hitherto been at a place called *Roxbury*, a mile from *Boſton*, went into town and received the ſmall-pox by inoculation. They had them with all the gentleneſs and moderation that was poſſible, neither of them having had an hundred puſtules, or being ſenſible of any fever worth mentioning; ſo that they did not find it neceſſary to keep their beds.

In *Auguſt* the natural ſmall-pox proved more mortal, and inoculation made a greater progreſs, the people continuing to come into the practice of it: A ſecond perſon, that died after inoculation, was an apothecary's houſekeeper, that was out of town, till an *Indian* maid got the diſtemper in the ſame houſe, and removed and died. Upon which this woman coming to town, her maſter undertook to perform the operation upon her (which by the bye was the firſt and laſt he ever performed) and on the third day after the inoculation, the ſmall-pox came out upon

upon her very full: From which it was plain, that she had taken the infection before, in the common way.

The third person, that died after inoculation, was a Gentleman of 45 years of age, that lodged in the same house with the Capt. and his wife; he was under great and extreme infirmity of body, and upon making the experiment he had not strength to go thro' with it.

The said Gentleman's sister was the fourth person that died upon this operation: She was about 40 years of age, of great indisposition of body, and weak, as was her brother.

The fifth, that died upon inoculation, was a woman servant in a house, where the whole family, to the number of 8, were inoculated at the same time: She lay in a cold upper room during her illness and was much neglected, the whole family being down together; so that she died merely for want of a little attendance. This happened in the town of *Roxbury*, where it is to be observed, that 13 men, masters of families, got the small-pox and all died, which induced the people to make use of inoculation, having been before much against it, and there were 43 men inoculated there, who all did well. The minister of the town was the first who put it in practice there, much against the mind of the people at first; tho' afterwards they were well pleased with it; seeing with what great success it was attended; and then whole families came into it and underwent the operation. There were in all at least 280 persons inoculated; and the Capt. supposes there might have been about 20 or 30 more, of which he could give no certain account.

An Account of a new Sort of Molosses, *made of* Apples, *and of the degenerating of* Smelts; *by Mr.* Dudley. Phil. Trans. N° 374. p. 231.

THE apple, that produces the molosses, is a summer-sweeting, of a middling size, pleasant to the taste and full of juice, so that seven bushels will make a barrel of cyder. The manner of making it is thus; you grind and press the apples, and then take the juice and boil it in a copper, till $\frac{3}{4}$ of it is wasted, which will be done in about six hours gentle boiling; and by that time it comes to be of the sweetness and consistency of molosses.

Some people scum the cyder as it boils, others do not, and yet there seems to be no great difference in the goodness.

These new molosses answer all the ends of that made of the sweet cane, imported from beyond sea. They serve not only for food and brewing, but are also of great use in preserving of cyder; two quarts of them put into a barrel of rack'd cyder will both preserve and give it a very agreeable colour.

The apple molosses were accidentally discovered a few years ago by Mr. *Chandler* at *Woodstock* in *New England*, a town remote from the sea, and where the *West India* molosses are dear and scarce: He ever after supplied his family with molosses out of his orchard, and his neighbours likewise did the same to their great advantage.

The country farmers run much upon planting orchards of these sweetings, for fatning their swine, and they assured Mr. *Dudley*, they make the best sort of pork: And he affirms, that the cyder made of them is better than that of other fruit, for taste, colour and keeping.

Two short miles from Mr. *Dudley*'s house there was a fine pond, half a mile over, having little or no communication with the sea: An ingenious man, some threescore years ago, for experiment sake, took a pail of large smelts from the river and put them into this pond, where they multiplied abundantly, but they degenerated to a very small sort; for, the river smelts Mr. *Dudley* supposes are full as large as those of the *Thames*, some of them weighing two ounces and $\frac{1}{2}$; whereas these small ones did not weigh five penny weight. The pond-smelts are thought to eat much better than the other, and they are very transparent and of a beautiful shining pearl-colour.

Observations on the Eclipse *of the* Moon, June 18, 1722; *and the Longitude of* Port Royal *in* Jamaica, *determined thereby*; *by Dr.* Halley. Phil. Trans. N° 375. p. 235.

THE eclipse of the moon, that happened in *June* 1722, was so far hid by the cloudy sky, that neither Dr. *Halley* nor any other in or about *London*, could furnish an observation thereof, worthy to be laid before the *Society*. But the same having been well observed at *Jamaica* by Capt. *Candler*, commander of his Majesty's ship the *Launceston*, and at *Berlin* by M. *Christfried Kirch*, astronomer of the *Royal Academy of Sciences* there; the Dr. prefixes to their accounts that little he himself was able to note concerning it.

June 18. in the morning, having perfectly rectified his clock, so as to shew the apparent time; neither the transit of the moon over

over the meridian, nor the beginning of the eclipſe, which ſoon follow'd, could be obſerv'd by reaſon of a very thick cloud.

At 13h 12′ app. time a ſmall portion of the moon's body was ſeen thro' a very little *hiatus* in the cloud, by which glimpſe the Dr. could only be aſſur'd, that the eclipſe was not yet total.

At 13h. 22′ by ſuch another view he was ſatisfied, that it was now become total; but in a moment it again diſappear'd, till 14h. 49′ 10″, when the cloud beginning to break, he got time to meaſure with the micrometer the lucid parts, now recover'd in the moon's diameter, which he found 14′, tho' not ſo well as he could wiſh, by reaſon of a thinner ſort of cloud, which perpetually interpos'd, and render'd the edge of the ſhadow ſomewhat dubious.

At 15h. 15′ the moon was pretty well got out of the thick cloud; but being very low and the day-light becoming ſtrong, ſhe ſhone very faintly and the ſhadow became worſe and worſe defin'd.

From 15h. 26′ to 15h. 27′ T. app. the Dr. doubted of the end, and he is poſitive it did not exceed the 27′. It ended over againſt the north part of *Palus Mæotis Hevelii*, much about the middle of the weſtern or right hand limb of the moon, ſhe being then very near ſetting.

Capt. *Candler*, being at that time at *Port Royal* in *Jamaica*, had much better fortune and a ſerene ſky from the beginning to the end; and having us'd due care to be aſſur'd of the times by altitudes, taken with an inſtrument of three foot radius, the reſult of his obſervations is, as follows.

	h	′	″
The eclipſe began	6	59	10
Immerſion	8	7	50
Emerſion	9	11	00
The end	10	19	40
Whence the middle	8	39	25

And ſuppoſing the eclipſe to have ended at *Greenwich* at 15h 26′ and $\frac{1}{2}$; the difference of Long. between *Port Royal* and *Greenwich* will be 5h 6′ 50″ or 5h 6′ and $\frac{1}{2}$ from *London*, that is 76° 37′ and $\frac{1}{2}$.

M. *Kirch*, being in a more eaſterly meridian, could obſerve nothing of the emerſion, but he carefully noted the time

time of the beginning and immersion, as he observ'd them at *Berlin*; viz. the beginning of the eclipse at 12h 59′ 55″, and the immersion at 14h 8′ 8″. Now by comparing several observations, made at both places, the Dr. formerly concluded *Berlin* to be 54′ of time, or 13° and ½ more easterly than *London*: Wherefore, at *London* it began at 12h. 5′ 55″, and immerged at 13h. 14′ 8″; that is, the beginning was later there than at *Jamaica* 5h. 6′ 45″, and the emersion later 5h. 6′ 18″, punctually agreeing with what resulted from the Dr's observation of the end, as aforesaid, and sufficiently with what he had long since determined from observations sent him from *Jamaica* by Mr. *Charles Boucher*.

The Longitude of Carthagena *in* America; *by the Same.*

Phil Trans. N° 375. p. 237.

AMONGST several observations, sent from *Carthagena* in *America*, and made by Colonel *Don Juan de Herrera*, chief Engineer of that city, Dr. *Halley* found one immersion of the first satellite of *Jupiter* into his shadow, observ'd there by a 17 and ½ foot telescope on *April* 9. 1712, O. S. 15h 58′ 44″ apparent time; as also two emersions of the same satellite, on *July* 5, 11h 23′ 41″ and *July* 21, 9h 42′ 17″ 1723, O. S. all which tally with observations, made at *Wansted* by Dr. *Pound* and Mr. *Bradley*, who observ'd there the very next eclipses to all the three; that is, the immersion by a 15 foot telescope on *April* 11. 15h 28′ 40″ equat. time, or 15h 30′ 25″ app. time; and the first emersion *July* 7. 10h 59′ 28″ equat. time by the reflecter, and 18″ after, or 10h 59′ 46″ by the 15 foot telescope, that is, 10h 54′ 12″ app. time. The other was observ'd at *Wansted*, *July* 23, 9h 19′ 10″ equat. time, both by the reflecter and 15 foot telescope; that is, at 9h 13′ 35″ app. time. Substract from each of these one period of this satellite or 1° 18h 28′ 36″, and *April* 9, 15h 58′ 44″ at *Carthagena* will be 21h 1′ 49″ of the same day at *Wansted*, and the difference of meridians 5h 3′ 5″; likewise by the first emersion *July* 5, 11h 23′ 41″ at *Carthagena*, was at *Wansted* 16h 25′ 36″ of that day; whence the difference of meridians 5h 1′ 55″. But by the last emersion *July* 21, 9h 42′ 17″ at *Carthagena* was 14h 44′ 59″ at *Wansted*; whence *Wansted* is 5h 2′ 42″ more easterly than *Carthagena*; and taking the medium of all three, 5h 2′ 34″ or 75° 38′ may be taken for the true difference of Long. that is 75° and ½ from *London*, which compar'd with Capt. *Candler's*

Candler's obſervation, ſhews *Carthagena* to be about 20 leagues to the eaſtward of *Port Royal* in *Jamaica*.

Obſervations on a Comet *ſeen at* Berlin, *from the* 18th *of* January *to the* 5. *of* February 1718, N. S. *By* Chriſtfried Kirch. Phil. Tranſ. N° 375. p. 238. *Tranſlated from the* Latin.

HERE M. *Kirch* hints, that the obſervations of this comet, publiſhed in *Nov. Liter. Lipſien.* are not accurate; for, on the 23. of *Januury* in the morning, the comet form'd an iſoſceles triangle with θ and φ of *Caſſiopeia* and not with ♌ and φ; and in the evening, the φ of *Perſeus*, the comet and the θ of *Caſſiopeia* were as to ſenſe in a ſtreight line.

M. *Kirch* obſerv'd the comet from the 18th of *Januury* to the 5th of *February*: The following table exhibits its places by obſervations till 10 in the evening.

	Longitude. ° ′	Latitude. ° ′
18 *Jan.*	27 26 *Cancer*	69 18 S.
21 *Jan.*	16 25 ½ *Taur.*	48 42 S.
23 *Jan.*	9 28 ½ *Taur.*	39 45 S.
26 *Jan.*	5 25 ½ *Taur.*	32 55 S.
27 *Jan.*	4 41 *Taur.*	31 24 S.
28 *Jan.*	4 4 *Taur.*	30 13 S.
30 *Jan.*	3 4 *Taur.*	28 23 ½ S.
31 *Jan.*	2 43 *Taur.*	27 40 S.
1 *Feb.*	2 25 *Taur.*	27 1 S.
2 *Feb.*	2 10 *Taur.*	26 22 S.
5 *Feb.*	1 39 *Taur.*	24 53 S.

The

The path of the comet pass'd above the back of *Ursæ Minor*, near the pole star, thro' the legs and knees of *Cepheus*, *Cassiopeia* and *Andromeda*; its descending node was in 21° and ½ of *Aries*, with same mutation; the angle of the cometic orbit and ecliptic was about 69° and ½, with some variation too; the path of the comet was almost 2° from the pole of the world and intersected the equator in 20° and ½ from the equinoctial point; its *perigæum* was in 6° 6′ of *Virgo* with 62° 7′ N. Lat. The comet was in its *perigæum* on the 18th of *January* 3h 9′ in the morning; its diurnal motion in its orbit was 22° 8′ in the *perigæum*, *viz.* 12 hours before and 12 hours after the *perigæum*; but on the last days of its appearing 32′. Supposing the earth at rest and the comet moving in a right line, the motion of the comet was 391 such parts, whereof the least distance of the comet from the earth is 1000. M. *Kirch* could determine nothing with certainty about the parallax of the comet, only that it was a great deal higher than the moon; and he conjectures with some probability that it mov'd within the orbits of the planets; nay, that in its *perigæum* it was much nearer to us than the sphere of *Mars*. For, suppose the semidiameter of the earth's orbit be 10,000 parts, the diurnal motion of *Mars* will be 139 or 140 such parts: If we suppose the comet to have been in the orbit of *Mars* with 62° 7′ Lat. and 22° 8′ diurnal motion, its velocity would be 2847 parts, supposing at the same time it was in opposition to the sun; but since the difference of Long. of the sun and comet in its *perigæum* was only 141° 40′, the diurnal motion of the comet becomes 3200 parts, and the proportion of the motion of the comet to the motion of *Mars* as 23 to 1: Wherefore, he collects, that the comet mov'd within the sphere of *Mars*. But should any one suppose the comet to have moved within the orbit of *Saturn* it should then have a velocity, that would be to the velocity of *Saturn* as 600 to 1, and that in one day it would run over a greater space, than the earth does in half a year; not to mention the diameter of the comet, that should be no less than three diameters of the sun.

M. *Kirch* comparing this comet with others, found that observ'd by *Regiomontanus* in *January* and *February* 1472 or 1475 moved in a tract not very different therefrom; for, it pass'd thro' *Ursa minor*, the thighs of *Cepheus*, the breast or neck of *Cassiopeia* and the girdle of *Andromeda*; and its greatest velocity in a day was 40°. Another comet was ob-

serv'd

ſerv'd in 1556, whoſe nodes *Camerarius* placed in 11° of *Libra* and *Aries* and which paſs'd near the feet of *Urſa minor*, thro' *Cepheus*, above *Caſſiopeia* and thro' the upper parts of *Andromeda*, with a very ſwift motion in its *perigæum*. And if *Regiomontanus* obſerv'd a comet in 1475 (which Aſtronomers very much doubt) there would be a ſurpriſing coincidence between theſe three comets: For, the interval between the former and middle comet would be 81 years, and between the middle and laſt comet 162 years, that ſo the revolution of the comet might be 81 years; and the hiſtory of other comets would alſo agree very well with theſe.

An Account of the Appearance of ſeveral Arches of Colours, contiguous to the inner Edge of the common Rainbow, obſerv'd at Petworth *in* Suſſex; *by Dr.* Langwith. Phil. Tranſ. N° 375. p. 241.

WHEN the primary rainbow has been very vivid, Dr. *Langwith* has obſerv'd therein, more than once, a ſecond ſeries of colours within, contiguous to the firſt, but far weaker, and ſometimes a faint appearance even of a third: Theſe increaſe the rainbow to a breadth much exceeding what has hitherto been determined by calculation; and an ingenious friend of the Dr's taking notice of this appearance, was much ſurpris'd at it, as thinking it not reconcileable with the theory.

The Dr. obſerv'd ſomething of the ſame nature, tho' not in the ſame degree of perfection, with that in the foregoing account, as follows.

March 22, 1721-22, a little before 6 o'clock in the evening, wind at N. W. by W. there was obſerv'd at *Petworth* a lively, diſtinct, primary rainbow, the inner and purple colours of which had a far greater mixture of red therein, than the Dr. could ever obſerve in Sir *Iſaac Newton*'s oblong *ſpectrum*; under this was a ſpace of a breadth, conſiderably leſs than that of the limb of the rainbow, in which the Dr. could not diſtinguiſh any colours; ſtill lower was a faint interrupted arch of red, inclining to purple, which appear'd and vaniſh'd ſeveral times, while he was intent upon obſerving it.

As to this phænomenon the Dr. ſuſpects, that the extraordinary redneſs in the purple of the firſt rainbow, is owing to the mixture of the red rays of a ſecond ſeries of colours with the purple rays of the firſt; that the colourleſs ſpace conſiſted of rays, which are too weak to affect the ſight

with distinct colours; and that the innermost broken arch was the end of a second and beginning of a third iris.

As in this last account the Dr. observ'd the purple of the second iris, without the other colours, so he observ'd the other colours, but not very distinct, without the purple; and he could never observe any more than one series of colours near the horizon.

March 27, 1722, about a quarter before 6 o'clock in the evening, wind S. W. the Dr. observ'd at *Petworth* one of the finest rainbows he ever beheld in his life.

The first series of colours was as usual, only the purple had a far greater mixture of red therein, than he had ever observ'd in the prismatic purple; under this was a coloured arch, in which the green was so predominant, that he could not distinguish either the yellow or the blue; still lower was an arch of purple, like the former, highly saturated with red, under which he could not distinguish any more colours.

The Dr. view'd this delightful object for a considerable time, without that vanishing and returning of colours, observ'd in the last.

The order of the colours in this compounded rainbow was red, yellow, green, blue, a mixture of purple and red, green (or rather a mixture of yellow, green and blue) a mixture of purple and red.

The Dr. now began to imagine, that the rainbow seldom appears very lively without something of this nature, and that the suppos'd exact agreement between the colours of the rainbow and those of the prism, is the reason that it has been so little observ'd.

The Dr. afterwards observ'd this phænomenon at *Petworth* in very great perfection, as follows.

August 21. 1722, about half an hour past 5 o'clock in the evening, the weather temperate, wind at N. E. the colours of the primary rainbow were as usual, only the purple inclin'd very much to red and was well defin'd; under this was an arch of green, the upper part of which inclin'd to a bright yellow, the lower to a more dusky green, under this were alternately two arches of reddish purple and two of green, under all a faint appearance of another arch of purple, which vanish'd and return'd several times so quick, that he could not steadily fix his eyes upon it:

The order of the colours was, as follows.

1. Red, orange-colour, yellow, green, light blue, deep blue, purple. 2. Light-green, dark-green, purple. 3. Green, purple. 4. Green, faint vaniſhing purple.

Here are four orders of colours, and perhaps the beginning of a fifth: For, the Dr. does not doubt, but what he calls the purple, is a mixture of the purple of each of the upper ſeries with the red of the next below it, and the great a mixture of the intermediate colours. A clergyman and four other gentlemen in company, viewing the colours attentively, agreed, that they appeared in the manner juſt now deſcrib'd.

There are two things worthy obſervation, as they may, probably, direct us in ſome meaſure to the ſolution of this curious phænomenon, as,

1. That the breadth of the firſt ſeries ſo far exceeded that of any of the reſt, that as near as the Dr. could judge, it was equal to all of them taken together.

2. That he never obſerv'd theſe inner orders of colours in the lower parts of the rainbow, tho' they were often incomparably more vivid than the upper parts, under which the colours appeared. The Dr. obſerv'd this ſo very often, that he could hardly think it accidental; and if it ſhould prove true in general, it will bring the diſquiſition into a narrow compaſs: For, it will ſhew, that this effect depends upon ſome property, which the drops retain, whilſt they are in the upper parts of the air, but loſe as they come lower, and are more mix'd with each other.

A Method of procuring the Small-Pox *in* South Wales; *by Dr.* Perrot Williams. Phil. Tranſ. N° 375. p. 262.

THE method of communicating the ſmall-pox has been commonly practis'd by the inhabitants of *Pembrokeſhire*, in *South Wales*, time out of mind, tho' by another name, *viz.* that of buying the diſeaſe, as the Dr. was inform'd by ſeveral, who procur'd the diſtemper by that means. A married woman in the neighbourhood of *Haverford-weſt* practis'd this method on her daughter, by which means ſhe had the ſmall-pox favourably, and was in perfect health; notwithſtanding ſhe ever after convers'd without reſerve with ſuch as had that diſtemper.

In order to procure the diſtemper, they either rub the matter, taken from the puſtules, when ripe, on ſeveral parts of the ſkin of the arms, &c. or prick thoſe parts with pins, &c.

&c. previouſly infected with the ſame matter: And notwithſtanding they omit the neceſſary evacuations, ſuch as purging, &c. yet as the Dr. was inform'd, they generally come off well enough; and what is remarkable is, that he could not hear of one inſtance of their having the ſmall-pox a ſecond time.

One Mr. *Owen* a gentleman of this country told the Dr. that above 20 years before, when at ſchool, he and ſeveral of his ſchoolfellows, infected themſelves at the ſame time, from the ſame perſon, and that not one of them miſcarried.

The method the above mentioned gentleman took was as follows; with the back of his penknife he rubb'd the ſkin off his left hand, till the blood began to appear; he applied the variolous matter to that part, which by degrees growing inflam'd, about a week after, he fell into the ſmall-pox.

The Dr. converſed with ſeveral others, who made the like experiments on themſelves, and they all poſitively affirm'd, they never had the ſmall-pox a ſecond time.

A farther Account of procuring the Small-Pox; *by the Same.* Phil. Tranſ. N° 375. p. 264.

MR. *Owen* was about 15 years of age, when he made the experiment upon himſelf, and he undoubtedly had the genuine ſmall-pox, the ſigns of them on his face, and the mark on his hand, where he had applied the matter, being very viſible, as to put that matter beyond diſpute. The Dr. had been ſo often aſſur'd of the truth of procuring the diſtemper in this manner, that he is entirely ſatisfied it has been a cuſtom immemorial in theſe parts; and not only practis'd by boys at ſchool, but alſo by many other grown perſons of both ſexes, who conſequently were capable of diſtinguiſhing the ſmall-pox from other diſtempers.

A young woman 23 years of age, told the Dr. that about eight or nine years before, in order to infect herſelf, ſhe held 20 pocky ſcabs (taken from one towards the latter end of the diſtemper) in the hollow of her hand a conſiderable time; that about 10 or 12 days after, ſhe ſickened and had upwards of 30 large puſtules on her face and other parts; and that ſhe afterwards freely convers'd with ſuch, as had the ſmall-pox.

To make it appear, that inoculation is a preſervative againſt receiving the ſmall-pox a ſecond time, the Dr. caus'd his two ſons, who had been inoculated, not only to ſee, but even to handle a child, dying of the moſt malignant ſort of ſmall-pox, and yet they continued in perfect health.

Upon

Upon a very exact enquiry the Dr. found, that out of 227 who had the ſmall-pox in the natural way, in *Haverford-weſt* and a neighbouring pariſh, ſince the beginning of *June* 1722, 52 had died.

A farther Account of buying the Small-Pox; *by Mr.* Richard Wright. Phil. Tranſ. N° 375. p. 267.

IN *Wales* the cuſtom of buying the ſmall-pox, as it is called, is a common practice and of very long ſtanding, perſons of unqueſtionable veracity and of advanc'd age affirming, that they have had the ſmall-pox communicated to themſelves this way, when about 16 or 17 years of age (being then very capable of diſtinguiſhing that diſtemper from any other) and that they have parted with the matter, contained in the puſtules to others, which produced the ſame effects. Two large villages near the harbour of *Milford*, namely, St. *Iſhmael's* and *Marloes*, are more famous for this cuſtom than any other: The old inhabitants of thoſe villages (with which they abound, being in a healthful ſituation) affirm, that it has been a common practice with them, time out of mind; and what was more remarkable, one *William Allen* of St. *Iſhmael's*, 90 years of age, declar'd, that this practice was us'd all his time; that he very well remember'd his mother's telling him, it was a common practice all her time, and that ſhe got the ſmall-pox that way.

Theſe together with the many other informations Mr. *Wright* had from almoſt all parts of *Pembrokeſhire*, confirm him in the belief of its being a very ancient and uſual practice among the common people; and to prove, that this method is ſtill continued in *Wales*, Mr. *Wright* gives the relation of one *Joan Jones* a midwife, 70 years of age, who ſolemnly declar'd, that about 54 years before, having then the ſmall-pox, one *Margaret Brown*, then about 12 or 13 years of age, bought the ſmall pox of her; that the ſaid *Margaret Brown* was ſeis'd with the ſmall-pox a few days after, and that ſhe had not the ſmall-pox a ſecond time; ſhe farther affirm'd, that ſhe had known this way of procuring the ſmall-pox, practis'd from time to time upwards of 50 years; that it was lately us'd in her neighbourhood, and that ſhe knew but of one dying of the ſaid diſtemper, when communicated after the method aforeſaid, the perſon who miſcarried (a young woman about 20 years of age) having procur'd the diſtemper from a man, then dying of a very malignant ſort of ſmall-pox.

As

As to the manner of communicating the infectious matter to the blood, by ſcraping the ſkin with a penknife, and ſo rubbing in the matter; that was only the caſe of one particular gentleman, Mr. *Owen*, whom Mr. *Wright* heard ſeveral times poſitively affirm, that he bought the ſmall-pox, when at ſchool, and that he gave three pence for the matter contain'd in 12 puſtules: It is certain that hundreds in this country have had the ſmall-pox this way, and not one ſingle inſtance can be produced of their ever having them a ſecond time.

Experiments to prove, that the force of moving Bodies is proportional to their Velocities; by Dr. Deſaguliers. Phil. Tranſ. N° 375. p. 269.

M. *Leibnitz* was the firſt who oppos'd the receiv'd opinion about the quantity of the force of moving bodies, by ſaying, that it was to be eſtimated by multiplying the maſs of the bodies, not by their velocity but by the ſquare of the velocity: But inſtead of ſhewing any paralogiſm in the mathematical demonſtrations, made uſe of to prove the propoſition, or any miſtakes in the reaſonings from the experiments made to confirm it, he uſes other mediums to prove his aſſertions, vide *Acta Erudit. ad ann.* 1686, *p.* 162. and without any regard to what others had ſaid on that ſubject, brings new arguments, which the learned Dr. *Clarke* has fully anſwer'd in his fifth letter to him: M. *John Bernouilli*, *Wolfius Hermannus* and others have follow'd and defended M. *Leibnitz*'s opinion and in the ſame manner; ſo that what is an anſwer to him, is ſo to them.

Polenus, Profeſſor at *Padua*, has acted in the ſame manner in the experimental way, making ſome experiments to defend M. *Leibnitz*'s opinion, vide *Polen. de Caſtellis p.* 56, 57, &c. without ſhewing thoſe to be falſe, that are made uſe of to prove the contrary; and an ingenious profeſſor abroad (who was of the opinion commonly receiv'd, and in his writings had demonſtrated it in the uſual way, vide *Graveſande*'s *Introduct.* Vol. 1. N° 132, confirming it with the common experiments, made in that caſe) happening to make ſome experiments, like thoſe of *Polenus*, has drawn concluſions from them to ſhew the force of moving bodies to be proportional to the ſquare of their velocity; and being wholly come over to that opinion, endeavours to deduce it from phyſical principles.

As

As there can hardly be any thing ſaid either new or better than has been already ſaid, to ſhew the force abovemention'd to be proportional to the maſs, multiplied into the velocity; Dr. *Deſaguliers* only repeats the ſubſtance of what others have ſaid, and makes ſome old experiments over again: But then he conſiders ſome circumſtances, that perhaps have been overlooked; and laſtly, by a new experiment, he endeavours to ſhew, what has led into an error ſome of thoſe, who defend the new opinion.

If a man with a certain force can move a weight of 50 pounds, thro' a ſpace of 4 feet, in any determinate time; it is certain he muſt employ twice that force to move 100 pounds weight, thro' the ſame ſpace in the ſame time. But if he uſe but the ſame force, he will move the 100 pounds weight, but 2 feet in the ſame time; whereas, the 100 pounds weight contains 2 fifty pounds, if each of them have 2 degrees of velocity given it, it will exactly require the ſame force, that would give one of them 4 degrees of velocity: Hence it appears, that the force is proportional to the maſs, multiplied into the velocity.

Experiment 1. Let the balance AB Fig. 14. Plate VII. whoſe *fulcrum*, or center of motion, is at C, be ſo divided, that the *brachium* AC be but $\frac{1}{4}$ part of the *brachium* CB; it is known to all converſant in mechanics, that a weight of 100 pounds at A, will keep in *æquilibrio* a weight of 25 pounds, ſuſpended at B, where it will have a velocity 4 times greater than that of the weight at A: For, not only when the balance is horizontal, there will be an *æquilibrium*; but when the balance is put in motion, it will return to an *æquilibrium* in an horizontal poſition; the equal and contrary forces, applied at each, deſtroying one another; Whereas, if the forces were as the maſs, multiplied into the ſquare of the velocity, the 25 pounds weight ſhould have been ſuſpended at D, only twice as far from C, as the weight at A; and in general let the make of the engine be what it will; let the mechanical powers be combined in any manner; when 2 heavy bodies, by means of a machine, act upon each other in different directions, if their velocities are reciprocally as their maſſes, they will deſtroy each other's forces and come to reſt.

As this is true in reſpect of mechanical powers, ſo it is alſo in reſpect to the ſhock or blow, given by falling bodies: An heavy body, falling with an accelerated motion, goes thro' a ſpace of 1 foot in $\frac{1}{4}$ of a ſecond, and acquires a velocity, which would

would carry it 2 foot in the ſame time with an uniform motion; the ſame body falls thro' a ſpace of 4 foot in $\frac{1}{2}$ a ſecond, and acquires a velocity, that would with an uniform motion carry it 8 foot in $\frac{1}{2}$ a ſecond: Therefore, as the time of the fall thro' a ſpace of 4 foot is twice the time of a fall thro' one of 2 foot, the velocity in the latter caſe is double that of the former; and conſequently, the blow that the body will give, will be double.

Exp. 2. Let the weight P of one pound Fig. 15. be placed in the ſcale, ſuſpended at the extremity A of the balance A B, which bears upon the *gnomon*, or iron ſupporter *k h i*: Then if the weight C be let fall from D, or 1 foot, it will by its ſtroke on the end of the beam B, raiſe up the oppoſite end A with the weight P, ſo high, that the ſpring *g h* will fly from the button *i*, which kept it ſtreight and upright before the ſhock. If the weight P be of 2 pounds, it cannot be raiſed by the fall of C from any height leſs than F or 4 foot: Whereas, if the force of the ſhock were proportional to the ſpace, without any regard to the time, as M. *Leibnitz* and his followers affirm, P ought to be raiſed, when C falls only from E, or 2 foot, which never happens; or if the ſtroke were proportional to the maſs, multiplied into the ſquare of the velocity, when C falls from F, then P might weigh 44 pounds; whereas the experiment will never ſucceed under thoſe circumſtances.

It is objected, that the blow cannot be always direct, and that the ſtring which goes thro' the hole in the falling weight, to guide it in its fall, cauſes a ſenſible friction; and therefore, that ſomething of the force is thereby loſt. But we are to obſerve, that if that were all, there would be no need of raiſing up C in the ſecond caſe quite up to F, inſtead of E; whereas, in fact it muſt always be raiſed beyond F, to allow for the friction, that hinders it from producing a double effect in falling from F. For, if the experiment be repeated an hundred times, the weight P, when of 2 pounds, will never be raiſed by letting C fall from any place between E and F.

Exp. 3. If (in order to avoid friction) inſtead of a blow, ſtruck upon the end B, by the falling body, the ſaid body C be faſtened to a pretty long ſtring, tied to the button *m*, as at *c*, and firſt lifted up 1 foot and then let fall; ſo that in falling 1 foot, it may pull down B, and lift up A with the weight P of 1 pound; whenever P is 2 pounds, C muſt fall from a height, greater than *f* or 4 foot; otherways it will not

not raiſe the *brachium* A, eſpecially, if it be let fall between *e* and *f*.

Exp. 4. The Dr. took the weight C of 17 ounces *Troy*, which was a round ball of lead, with a hole thro' the middle, and having paſſed the ſtring N—X thro' it, before it was faſtened to the hook X, he placed the whole machine in ſuch a manner, that the ſtring being ſtretched by the weight N, went thro' the hole of the weight C, and likewiſe thro' the hole of the *brachium* B, upon which C lay, without touching the ſides of the hole either in the weight or balance; then having put ſuch a weight P in the oppoſite ſcale, as C, falling from the height of 1 inch, was able to raiſe high enough, to let looſe the ſpring *g h* from the button *i*; he added to P another weight equal to it; and then letting C fall along the ſtring, that guided it, from an height of 2 inches, then of 3, and then exactly of 4, it would not raiſe the double weight P to the former height; but falling from 5 inches or a little higher, it raiſed it up.

Exp. 5. Leaving every thing, as it was before, he changed the weight C for another leaden ball of twice the weight, which falling from 1 inch, raiſed the double weight P to the uſual height; then changing the weight P in any proportion, whatever height was required for the heavieſt ball C or C_2, to fall from, in order to raiſe the weight at P; more than 4 times the height was required for the firſt ball C, to raiſe the ſame weight ſo high, as to let looſe the ſpring.

Exp. 6. He tried the experiment with the weight C, ſuſpended at the ſtring *mc* (as in *Exp.* 3.) and a fall from a height of 5 or near 5 inches, was required to raiſe double the weight in the oppoſite ſcale, that a fall from 1 inch would raiſe; only here the height above 4 inches was not ſo great, as in the former experiment, the friction being ſomething leſs. Then he ſuſpended the large ball C or C_2 by the ſtring *mc*, and when by falling 1 inch it raiſed the weight P, the ſmall weight C could not produce the ſame effect, without falling from a greater height than 4 inches.

It is here to be obſerved, that which way ſoever theſe experiments are tried, the objections, ariſing from the friction, do noways ſerve to confirm the new opinion, becauſe they ſhew, that upon account of the friction, the heights muſt be ſomewhat more than in a duplicate proportion of the velocities, but never leſs, to give a blow with the ſame body in proportion to the velocity.

That the *momentum* of bodies is in proportion to the mass, multiplied into the velocity, is also most evidently shewn from the congress of elastic bodies, as has been demonstrated by Sir *Isaac Newton* in his *Principia* in the *Corollaries* to the laws of motion. Dr. *Desaguliers* had often tried the experiments there mentioned with balls of ivory and balls of glass; and some of them with two balls of steel, of two ounces each; and he found every thing answer, allowing for the want of perfect elasticity in the bodies. But now, as the objections to the received opinion were renewed, the Dr. was willing to repeat the experiments with the utmost accuracy; and therefore, as ivory balls are not equally dense in all their parts and glass balls break after 2 or 3 strokes; he caused steel balls to be nicely turned and made as hard (as the workmen call it) that is, as elastic as possible, and their weights were precisely as follows; 2 balls of 12 ounces *Troy* each, 1 of 6 ounces, 1 of 3, 1 of 2, and 1 of 8 penny-weight; then making pendulums of these balls, and hanging them upon the machine, contrived by *Mariotte* for the congress of bodies, and improved by Dr. *Gravesande*, vide *Introd.* N° 171. Vol. I. he measured 57 inches and $\frac{1}{4}$ between the center of suspension and the center of gravity of the balls, and then every degree of the circle they described in their oscillation was 1 inch, and the degrees being marked upon a line of chords on a brass ruler above the balls, by their strings successively covering the cross lines of division, the degrees, the balls fell from, and those to which they rose, were very discernible to an eye placed at a convenient distance.

Exp. 7. He took the 2 balls 12 and removing each from the lowest point of their equal and respective circles, up to 4 inches or 4 degrees, he let them fall, so that they met at bottom, and were both reflected again to 4, the place from whence they fell.

Exp. 8. Every thing else being as before, instead of 1 of the balls 12, he took the ball 6; then letting 6 go from 8 degrees, and 12 from 4; after reflection 12 was driven up again to 4, as before.

Exp. 9. The ball 3 falling from 16 degrees met the ball 12, that fell still from 4, and after reflection 12 went up again to 4.

Exp. 10. The ball 2 falling from 6 degrees and 12 from 1°, 12 was reflected to 15 and when 2 fell from 12 degrees, and the ball 12 from 2, the 12 was reflected to 2.

Exp.

Exp. 11. The ball of 8 penny weight (which weighed but $\frac{4}{30}$ of the ball 12) falling from 15 inches or degrees, raiſed up 12 (that fell from $\frac{1}{2}$ a degree) to the ſame place again.

In all theſe experiments the error, or want of perfect reflexion, was greater in the ſmall balls than in the large ones, on account of their going thro' a greater arch of a circle, whereby they deviated more from a cycloid than the large balls; as likewiſe on account of the reſiſtance of the air, which muſt be greater, becauſe of the ſmall balls going thro' a larger arch, moving with more velocity, being ſuſpended by a ſtring, as thick as that of the large ones, and having more ſurface in proportion to their weight. But all the errors do not bring the phænomena any thing near what they ought to be, if the force of the bodies were, as the ſquare of the velocities, multiplied into their maſſes; for, then the ball 12 would have been driven to heights very different from what it roſe up to.

In the 8th experiment, the ball 12 ſhould have riſen to near 5 inches and $\frac{3}{4}$; for, the ball 6, falling with the velocity 8, muſt have had its force $= 8 \times 8 \times 6 = 384$; and then that the ball 12 might have the ſame force, or quantity of motion, it muſt riſe near to 5, 7; becauſe $5,7 \times 5,7 \times 12 = 389,88$.

In the 9th experiment, 12 ſhould have riſen to 8; for, the ball 3 muſt have had its force $= 16 \times 16 \times 3 = 768$; and if 12 received its whole force, it muſt have riſen to 8; becauſe $8 \times 8 \times 12 = 768$.

In the 2d part of the 10th experiment, 12 ſhould have riſen to near 5; becauſe $12 \times 12 \times 2 = 288$ and $5 \times 5 \times 12$ is but 300.

In the 11th experiment, the ball 12 (30 times heavier than the little one) muſt have gone to 2 inches $\frac{3}{4}$; becauſe the *momentum* of the little ball being equal $15 \times 15 \times 1 = 225$, that of the ball 12 muſt be $= 2,75 \times 2,75 \times 12 = 226$, *&c.*

It may be here alledged, that the *momentum*, with which the large ball comes upon the ſmall one, ſhould be ſubſtracted; but that will not mend the matter much: Tho' indeed, the difference will be leſs. For,

In the 8th experiment, if we ſubſtract $4 \times 4 \times 12 = 192$ from 389,88, there will remain 197,88, and the ball 12 will go but to 4: But then in experiment 9. if we ſubſtract the ſame N° 192 from 768, we ſhall have 576, which would carry 12 to near 7 degrees; becauſe $7 \times 7 \times 12 = 588$.

In the 10th experiment, there is only 48 to be ſubſtracted; and in the 11th only 15; and therefore, the velocity of 12

will very much fall ſhort of what is agreeable to the new opinion.

After the experiments made and what has been ſaid, till theſe conſequences are overthrown, no notice ought to be taken of any objections, or new experiments.

Animadverſions on ſome Experiments, relating to the Force of moving Bodies; *with two new Experiments on the ſame Subject; by the Same.* Phil. Tranſ. N° 376. p. 285.

IN the preceeding *Tranſaction* Dr. *Deſaguliers* demonſtrated by reaſon and experiments, that the *momentum* or force of moving bodies is always proportional to their maſs, multiplied into their velocity, and this is the opinion of the greateſt part of mathematicians and philoſophers. Now he comes to conſider the experiments, that have led ſome ingenious men into an error, as to this propoſition.

Polenus in his book *de Caſtellis* p. 56. N° 118. gives an account of his experiments, relating to this matter, in theſe words: 'I took a veſſel that had therein congealed tallow 6 inches 'deep, and fixed it to a level floor, in ſuch a manner, that the 'ſurface of the tallow, which was flat, ſhould every where be 'equally diſtant from the floor. I had cauſed to be made two 'balls of equal bigneſs; the one of lead, the other of braſs: 'the laſt of which was ſomewhat hollow in the middle, that it 'might weigh but 1 pound, whilſt the other weighed 2: Suſ'pending theſe balls from the cieling by threads, in ſuch a man'ner, that the lighter ball hung over the ſurface of the tallow, 'from twice the height that the heavier ball did; I cut the 'threads, and the balls falling perpendicularly upon the tal'low, by their fall made pits in the tallow, that were preciſely 'equal; the ball of 1 pound from the beginning of its fall 'till it came to reſt, going thro' a ſpace, expreſſed by number '2, produced an effect, equal to that, which the two pound 'ball produced, in falling thro' a ſpace, expreſſed by number '1. It follows, therefore, that we may look upon it as a ſet'tled truth, that the active forces (*vires vivas*) of falling bo'dies are then equal, when their proper weights are in a reci'procal ratio of the ſpaces, which the ſaid bodies deſcribe by 'their fall; and becauſe thoſe ſpaces are in the ſame ratio, as 'the ſquares of the numbers, expreſſing the velocities; it ap'pears by the experiment, that the active force (*vim vivam*) of 'the falling body, is that which is made up of the body itſelf 'multiplied into the ſpace, deſcribed in the fall, or into the

'ſquare

'ſquare of the number, that expreſſes the velocity of the 'body, at the end of the motion. This experiment I did 'not only make once, but ſeveral times, changing the balls, 'the diſtances and the body on which they fell; as for exam-'ple, making uſe of clay, or ſoft wax: And notwithſtanding 'theſe various ways of trying the experiments, the effects 'were conſtantly the ſame; which made me eaſily conclude, 'that there was always the ſame reaſon in nature for this 'phænomenon.'

Thus far *Polenus*; whoſe miſtake lies in this, *viz.* That he eſtimates the force of the ſtroke of the falling balls, by the depth of the impreſſion made in the tallow, clay, wax, or any yielding ſubſtance. But we muſt conſider, that when two bodies move with equal forces, but different velocities, that, which moves the ſwifteſt, muſt make the deepeſt impreſſion; whilſt the ſloweſt body communicates its motion to the clay round about; and therefore, does not ſtrike in ſo deep, as the ſwifter body, which puts in motion few parts of the clay, beſides thoſe that are before it, and which parts have ſo much leſs time to oppoſe this body's motion, as its velocity is greater than that of the other.

To make this plainer, let us ſuppoſe a door half open, and moving very freely on its hinges, if a piſtol be fired againſt it, the ball will go thro' the door, without moving it out of its place: But if we take a large weight of lead and throw it againſt the ſame door, with the ſame force as the piſtol bullet moved, the door will be removed from its poſition, and carried out of its place on its hinges by the ſtroke, becauſe in the firſt caſe, the motion of the bullet is communicated but to a few parts of the door, and in the laſt caſe, it is diffuſed all over it. Nay, the door will be moved by the ſtroke, even tho' there ſhould be a prominent part in the lead, that ſhould be no bigger than a piſtol-bullet, in order to ſtrike the door upon no more of its ſurface than the bullet had done.

For illuſtrating this farther the Dr. contrived the following experiment.

He cauſed a machine to be made (as repreſented by Fig. 16. Plate VII.) conſiſting of a baſe of wood A B, which could be ſet horizontal by means of three ſcrews, ſuch as SSS; upon this board or baſe there ſtood upright two parallel boards, about four inches wide and four inches aſunder, with the elbow-piece E F, ſliding behind one of them, ſo as to raiſe

its

its upper end F to any height desired. Between these boards six square frames of wood CC, &c. with paper extended thereon, could slide in, in an horizontal position: These paper diaphragms being thus placed, he suspended an ivory ball of about 1 inch and ½ in diameter, weighing something more than 1 ounce and ½, by a short thread under F; so that its center of gravity hung 4 foot over the first diaphragm; then cutting the thread, the ball fell upon the paper, and by its perpendicular stroke broke thro' that diaphragm and the 3 next under it. Then putting so much lead into the ball above mentioned (which was made hollow for that purpose) as to make it weigh twice as much as it did before; and bringing down F, to let it fall but from 1 foot; by its fall it broke thro' 2 diaphragms only. Making the experiment several times with different heights; but still keeping the proportion in height of 4 to 1, when the balls were as 1 and 2, the heavy and slowest ball broke thro' but half the number of papers. It happened sometimes, it is true, that there was some little difference, when the papers were not equally strong, or equally stretched; but the swiftest ball always broke thro' more papers than the slow one.

Now, tho' this does at first seem to confirm *Polenus*'s theory: yet when duly weighed, it proves no such thing: For, the lighter ball does not break thro' more papers; because it has more force, or a greater quantity of motion; but because each diaphragm has but ½ the time to resist the ball, that falls with a double velocity; and therefore, their resistance being as the time, as many more of them must be broken by the swift ball, as by the slow one.

What has been said, in this and the preceeding *Transaction*, seems to be a full answer to all objections, that allow the forces of moving bodies and their quantity of motion to be the same: But as there are now some philosophers, who distinguish that force from the quantity of motion, the Dr. adds something more for the clearing up of that point.

As the Dr. takes it, they call *vis viva* a force, whose effect is sensible, as the force of gravity, when it accelerates bodies in their fall; and *vis mortua*, a force, which being destroyed, produces no sensible effect; as the force of gravity acting upon a weight in one scale of a balance, when the weight cannot descend by reason of a counterpoise in the other scale. But certainly no man, that considers the thing attentively, would make that distinction: However, since *Polenus* allows, that the

quantity

quantity of motion in bodies is as the mass, multiplied into the velocity, or M V; but says, that the force, with which they act, which he distinguishes by the name of *vis viva*, is, as the mass, multiplied into the square of the velocity, or M V V: The Dr. made the following experiment to shew, that his notion is inconsistent; tho' all the phænomena of unequal weights, applied to a *statera*, so as to make an *æquilibrium*, might serve for that purpose; if it had not been objected, that the particular construction of the machine, hindered it from agreeing with the supposed theorem, *viz.* that the force is as the matter, multiplied into the square of the velocity.

Exp. Let two balls A and B (Fig. 17.) be joined by a string, which, going thro' the smooth hole C of an even table, and under the pulley P, suspends a weight W. It is plain, that upon letting go the balls A and B, from the places A and B, they will move towards C with the same force; because each of them will be drawn towards C by half the force of the weight W, whether the balls be equal or unequal.

1. The balls, being of ivory and weighing two ounces each were, at the same instant of time, let loose from A and B, each distant 12 inches from C, and both came to C at the same time: Here the equal forces will agree with the product of the masses into the velocities, or into the squares of the velocities; because $A \times 12 = B \times 12$, as well as $A \times 144 = B \times 144$.

2. If A be taken of 4 ounces weight and let go from D, or 6 inches; whilst B, still equal to 2 ounces, moves from 12 inches; both bodies will again meet at C: Therefore, here the equal forces must be expressed by the masses into the velocities, and not into their squares; for, tho' $A \times 6$ be equal to $B \times 12$ ($4 \times 6 = 2 \times 12$) $A \times 6 \times 6$ or 144 is but half of $B \times 12 \times 12$ or 288. Whereas, if the forces had been as *Polenus* affirms, B should have been let loose only from 8,4 inches.

3. When A is 6 ounces, it is let loose only from E, or 4 inches, to meet at C with B let loose from 12: For, then $A \times 4 = B \times 12$; whilst $A \times 4 \times 4$, or 96 is 3 times less than $B \times 12 \times 12$, or 288: So that, according to *Polenus*, B must have been let loose from 7: But in that case it comes sooner to C than A.

N. B. The weight W must be greater than that of both balls, least the friction of the table should spoil the experiment.

An

An Account of an Experiment made to aſcertain the Proportion of the expanſion of the Liquor in the Thermometer *with regard to the Degrees of Heat; by Dr.* Brook Taylor. Phil. Tranſ. N° 376. p. 291.

IT has been generally ſuppoſed, tho' not proved, that the expanſion of the liquor in the thermometer is proportional to the increaſe of heat. To determine this matter with certainty, Dr. *Taylor* made the following experiment.

He provided a good linſeed oil thermometer, which he marked with ſmall diviſions, unequal in length, but equal according to the capacity of the tube in the ſeveral parts of it, as all thermometers ought to be graduated. He likewiſe provided two veſſels of thin tin, of the ſame ſhape, and equal in capacity, containing each about a gallon. Then (obſerving in every trial, that the veſſels were cold, before the water was poured in, as alſo that the veſſel he meaſured the hot water with, was well heated thereby) he ſucceſſively filled the veſſels with 1, 2, 3, *&c.* parts of hot boiling water, and the reſt cold; and at laſt with all the water boiling hot; and in every caſe he immerſed the thermometer into the water, and obſerved to what mark it roſe, making each trial in both veſſels for the greater accuracy: And having firſt obſerved, where the thermometer ſtood in cold water, he found, that its riſing from that mark, or the expanſion of the oil, was accurately proportional to the quantity of hot water in the mixture, that is, to the degree of heat.

An Account of the Rattle-Snake; *by Mr.* Dudley. Phil. Tranſ. N° 376. p. 292.

THE rattle-ſnake is reckoned by the *Aborigines* to be the moſt terrible of all ſnakes, and the chief of the ſerpent kind; what cauſes the terror of them, is, undoubtedly, their mortal venom, and the enſign of it is their rattle; and it is moſt certain, that both men and beaſts are more afraid of them than of other ſnakes; and while the common ſnake avoids a man, this will never turn out of the way.

There are three ſorts of this ſnake, that are diſtinguiſhed by their colour, *viz.* a yellowiſh green, a deep aſh-colour, and a black ſattin.

The eye of this reptile has ſomething ſo ſingular and terrible, that there is no looking ſtedfaſtly upon him; one is apt almoſt to think, they are poſſeſſed by ſome Demon.

A rattle-

A rattle ſnake creeps with his head cloſe to the ground, and is very ſlow in moving; ſo that a man may eaſily get out of his way; his leaping and jumping to do miſchief, is no more than extending or uncoiling himſelf: For, they do not remove their whole body, as other animals do, when they leap; ſo that a man is in no danger of them, if his diſtance be more than their length; nor can they do any harm, when they are in their ordinary motion, till they firſt coil and then extend or uncoil themſelves, which are both done in a moment's time.

When a rattle-ſnake reſts or ſleeps, he is coiled; and they are obſerved to be exceeding ſleepy.

The tail of the rattle-ſnake is compoſed of joints, that lap over each other, ſomewhat like a lobſter's tail; and the ſtriking them upon each other forms that noiſe, which is ſo terrible to man and beaſt: The fierceſt noiſe is obſerved to be in fair clear weather; for, when it is rainy, they make none at all: For which reaſon the *Indians* do not care to travel in the woods, in time of rain, for fear of being among theſe ſnakes, before they are aware. Another circumſtance of their rattling has been obſerved, to wit, that if a ſingle ſnake be ſurpriſed and rattles, and there happen to be others near him, they all take the alarm, and rattle in like manner.

Mr. *Dudley*, would not pretend to anſwer for the truth of every ſtory he had heard of their charming or power of faſcination; yet he was abundantly ſatisfied from ſeveral witneſſes, both *Engliſh* and *Indian*, that a rattle-ſnake will charm both ſquirrels and birds from a tree into his mouth. Mr. *Dudley* was told by one of undoubted probity, that as he was in the woods, he obſerved a ſquirrel in great diſtreſs, dancing from one bough to another, and making a lamentable noiſe, till at laſt he came down the tree and ran behind a log; the perſon going to ſee what was become of him, ſpied a large ſnake that had ſwallowed him.

And Mr. *Dudley* is the rather confirmed in this relation, becauſe his own brother, being in the woods, opened one of theſe ſnakes and found two ſtriped ſquirrels in his belly, and both of them head foremoſt. When they charm, they make a hoarſe noiſe with their mouths, and a ſoft rattle with their tails, having the eye at the ſame time fixed on the prey.

Their common food is toads, frogs, crickets, graſshoppers and other inſects, but principally ground mice; and the rattle-ſnake again ſerves for food to bears; and even hogs will eat them without harm.

They are viviparous and generally bring forth about 12, and that in *June*. A friend of Mr. *Dudley*'s in the country, being desirous to discover the nature and manner of the generation of the rattle-snake, gave him the following account, *viz.* about the middle of *May*, the time when the rattle-snakes first come abroad, he took and opened one of them and found in the *matrix* 12 small globes, as big as a common marble, in colour like the yolk of an egg; in three or four days more he took and opened another, and then plainly perceived a white speck in the center of the yellow globe; in three or four days more he dissected a third, and discovered the head of a snake; and in a few days after that, $\frac{3}{4}$ of a snake was formed, lying round in a coil. In the latter end of *June* he killed an old one and took out perfect live snakes six inches long. In *September*, when the old ones take their young in and carry them to their dens, they are not quite a foot long. They couple in *August* and are then most dangerous.

As to other serpents or poisonous reptiles Mr. *Dudley* could not affirm; but he was satisfied the rattle-snake does not traject his poison; and that unless the skin be first broke or an incision made with his teeth, his venom can do no harm; for, his friend assured him, that he had made an experiment of it in this manner; he took the but-end of his gun; and set it upon four or five of them; and after they had bit it and left several drops of their poison, he wiped it off with his hand without any harm.

There are several remedies for the sting of a rattle-snake; among others, that which is much made use of, is a root, called blood root, so called, Mr. *Dudley* supposes, from the colour of the root, and the juice, which is red like blood. It grows in great abundance in the woods; they bruise the root and bind it over the place that is bit, to prevent the poison going farther; at the same time scarifying the place affected: Some of the root is likewise boiled, and the person bit drinks the water.

The rattle-snakes are generally from three to five foot long, and commonly exceed not 20 rattles; and yet Mr. *Dudley* was told by a man of credit, that he killed a rattle-snake, that had between 70 and 80 rattles, with a sprinkling of grey hairs, like bristles, over his body; he was full five foot and $\frac{1}{2}$ long, and as big as the calf of a man's leg.

They cast their skins every year, some time in *June* and turn it inside out, when they throw it off. It has also been observed, that the skin covers not only the body but likewise the head and eyes.

They

They generally den among the rocks in great numbers together; the time of their retiring is about the middle of *September*, and they do not come abroad, till the middle of *May*, when the hunters watch them, as they come out a ſunning, and kill them by hundreds.

Some Obſervations on Vipers; *by Dr.* Sprengell. Phil. Tranſ. N° 376. p. 296.

AT *Milan* Dr. *Sprengell* found a viper-catcher, who ſeldom was without 60, or more, vipers alive, kept together in a back room open a top; he had them from all parts of *Italy* and ſold them dead or alive, according to the uſes they were deſigned for. Having one day got a female viper, big with young, we catched ſome mice, and throwing in one at a time; amongſt all that number of vipers (which were upwards of 60) there was none of them that in the leaſt concerned himſelf about the mouſe, till the pregnant female viper and the mouſe interchanged eyes; whereupon the mouſe ſtartled; but the viper raiſed her head, and turn'd her neck into a perfect bow, the mouth open, the tongue playing, the eyes all on fire and the tail erect: The mouſe ſeem'd ſoon recover'd of his fright, would take a turn or two, and ſometimes more, pretty briſkly, round the viper, and giving at times a ſqueak, would run with a great deal of ſwiftneſs into the chops of the viper, where it gradually ſunk down the gullet. All this while the viper never ſtirred out of her place, but lay in a ring.

It is to be obſerved, that no viper will feed, when confined, except a pregnant female viper: The Dr. ſaw the ſame thing at *Bruſſels*, where a ſoldier had catched a large viper big with young.

A ſmall pig of 9 or 10 days old, was bit by the viper in the tail, and in four minutes time the tail was chopp'd off, the pig appearing to be ſick and dizzy, and the remaining part of the tail ſwell'd; but the bleeding, the Dr. ſuppoſes, ſav'd it; for, it was well again next morning: The ſame happened to another pig, which was bit in the fore-foot, and ſtaying ſeven minutes after the bite, the leg was cut off about two inches above the bite. After theſe two, three more were bit in ſeveral places, two of which died that night, and the third recovered, having given it, about five or ſix minutes after, 10 grains of emetic tartar.

This the Dr. tried afterwards upon dogs, bit by vipers and he found, that they all recovered upon the emetic tartar.

Observations on the Figures of Snow; *by Dr.* Langwith. Phil. Transf. N° 376. p. 298.

ON the 30. of *January*, 1723, something past 9 o'clock in the morning, weather cold, wind south westerly, but not very high, barometer above 30 inches, Dr. *Langwith* observ'd that pretty phænomenon of the star-like snow; and tho' upon comparing his observations with those of *Descartes*, Dr. *Grew* and Mr. *Morton*, he found he had but little to add upon the subject; yet he observed with a great deal of pleasure the progress of nature in this sort of crystallization, an account of which is, as follows.

ABCDEFG, Fig. 18. Plate VII. represent the several figures of snow: A and B are the most simple figures, of which the former is a roundish pellet of ice; the second a small oblong body, with parallel sides, and often as fine as a hair: Of this latter kind the flakes of snow chiefly consist; and tho' they look white to the eye, yet when viewed with a small magnifier of a microscope, they appear like so many transparent needles of ice, thrown together, without any manner of order.

The next figure of snow is C, in which the pellet has shot out six of those small bodies of equal length, and set at equal angles: Of this kind the Dr. observed a great many.

The next step in the crystallization is D, in which those bodies are lengthened and have shot out a great many more from their sides, at equal angles, but unequal lengths, as growing continually shorter and shorter, till they terminate in a point: He measured some of these, and found them to be $\frac{1}{4}$ of an inch in breadth. He observed but very few of them perfect; for, the collateral shoots were so exquisitely fine, as to be liable to be broken in their fall, or confounded together by the least degree of heat.

Of the next kind E, he observed a vast many, which, being examined by the microscope, plainly appeared to be nothing, but the former sort in disorder. The edges of these were in general very irregular; but some of them happened to be so indented, as to look like the jagged leaves of plants.

The next kind F had 12 points regularly disposed, and probably might consist of two of the former, so joined together, as to cut their angles equally.

Perhaps also those Mr. *Morton* describes (represented by G) as consisting of *radii*, which, instead of terminating in a point, grow bigger, as they advance from the center, might be form'd from

from two of the kind C, so join'd at the center as to cut each other's angles unequally; for, in the progress of the crystallization, these *radii* would quickly unite.

Lastly, that sort, which *Descartes* compares to roses, and of which he has given a figure in his treatise of meteors, may be nothing but the kind E, when the points are rounded off, by being gently thaw'd.

The Dr. proposes these things only by way of conjecture; because as the small drops of water may be impregnated with very different particles in the air, it is not easy to determine, whether these figures may not be the result of a crystallization, quite different from the former.

The Dr. observ'd but very few figures of 12 points, and those mostly imperfect in one respect or other.

Observations for four Years on the Aurora Borealis, *at* Lyn-Regis *in* Norfolk. Phil. Trans. N° 376. p. 390. *Translated from the* Latin.

THE author for four years together made some observations on the *Aurora Borealis*, at *Lyn-Regis* in *Norfolk*; he was not aware of the surprising phænomenon the sixth of *March* 1715 16; which made him the more sollicitous to watch the subsequent phænomena.

He very carefully delineated the first observation, which is here subjoin'd, as soon as he made it; and therefore he takes it to be but little wide of the truth: The rest of the descriptions are very accurate. The cause of these coruscations is not, he thinks, hitherto sufficiently discovered.

Friday Sep. 5. 1718. About 10 o'clock in the evening, the phænomenon (represented by Fig. 19. Plate VII.) was observ'd at *Lyn-Regis* to the north.

Saturday Sep. 6. About 8 and 10 o'clock in the evening, several columns of light were observed like those represented at *a a* in Fig. 19. but not so bright as the pyramids observ'd the night before, which were carried towards the east, but these towards the west.

Thursday Sep. 11. A great many columns of the same kind were again observ'd moving to the west.

Saturday Sep. 13. About 11. o'clock at night the *Aurora Borealis* was brighter and higher, and with longer pyramids than it was hitherto observ'd to have: This night it shone so bright, that one could read by it.

Saturday

Saturday Oct. 11. Between 10 and 11 o'clock at night the *Aurora Borealis* was brighter than ever, with pyramidal coruſcations interſpers'd; and not much unlike the phænomenon, obſerv'd *September* 5, it was ſo bright that one might read by it.

Friday December 19. Between 8 and 9 o'clock at night ſeveral rays of light, together with the *Aurora Borealis*, were obſerv'd to ariſe out of a black cloud, as it were; but that it was really no cloud appeared from the ſtars, being plainly ſeen thro' it; yet what elſe it was, could not ſo eaſily be determined: But what was chiefly obſervable was the very ſurpriſing motion of theſe lights: Theſe rays were continually in motion, and conſtantly ſhifting their places, directing their courſe now this way, and now that way, backwards and forwards, and ſometimes ſucceſſively advancing in the ſame direction, and ſometimes again impinging againſt each other with a tremulous and vibrating motion, and almoſt an incredible ſwiftneſs: Some rays were obſerv'd to extend as far as the zenith: The light was often obſerv'd to collect itſelf into a body, and ſo exhibit a ſurpriſing brightneſs, tinged with the colours of the rainbow; and again it was obſerv'd to dilate itſelf. This evening the moon ſhone bright.

Thurſday March 12. 1718-19. Between 10 and 11 o'clock the *Aurora Borealis* was obſerv'd again.

Friday March 27. The *Aurora Borealis* was obſerv'd again with various oblique radiations, as repreſented Fig. 20.

Monday October 26. 1719. It was likewiſe obſerv'd this evening between 7 and 8 o'clock.

As alſo *Monday Nov.* 9. 1719.

Sunday January 31. 1719-20. The *Aurora Borealis* was obſerv'd this evening from 7 till 10 o'clock higher than ever it had been before, and extending over half the heavens from eaſt to weſt; it was almoſt cover'd with ſeveral interſpers'd coruſcations, conſequently ſo bright that one might diſtinctly read by it.

Saturday Sep. 17. 1720. The ſame appearance.

Monday January 6. 1720-21. Between 7 and 8 o'clock at night the *Aurora Borealis* was again obſerv'd, with pyramidal coruſcations, ariſing on all hands from the zenith, as from a center and almoſt reſembling an *umbrella*.

Sunday January 12. 1720-21. The ſame appearance.

Monday September 11. 1721. About 9 o'clock at night.

Wedneſday

PLATE VIII.

Fig. II.

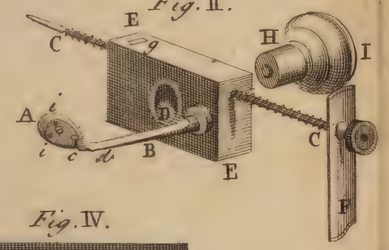


Fig. IV.

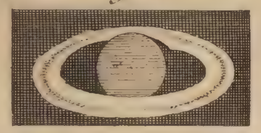


Fig. V.

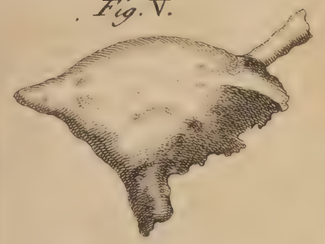


Fig. VI.

Fig. VII.

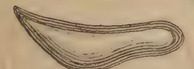


Fig. III.

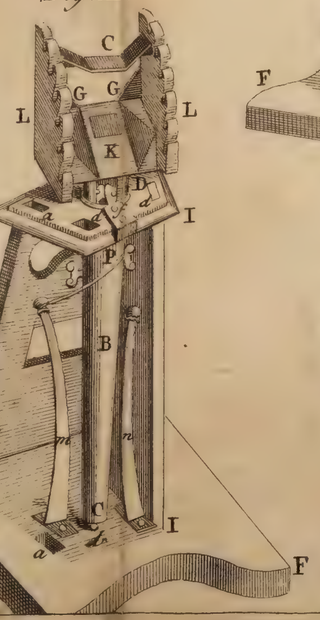


Fig. I.

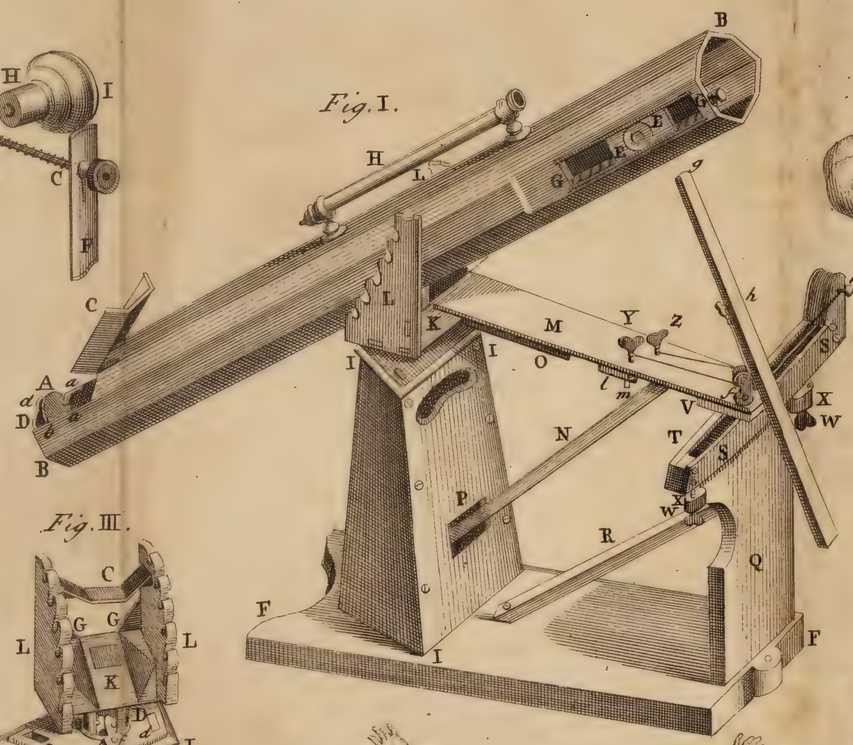


Fig. VIII.

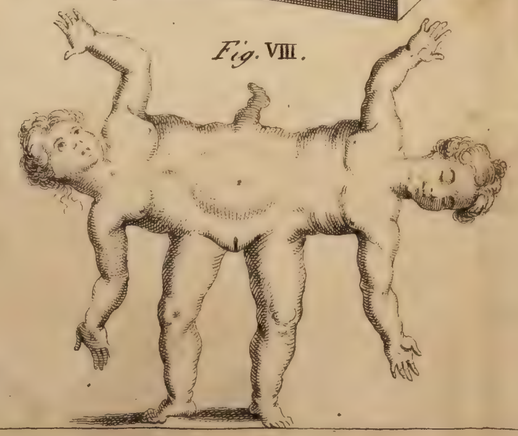


Fig. X.

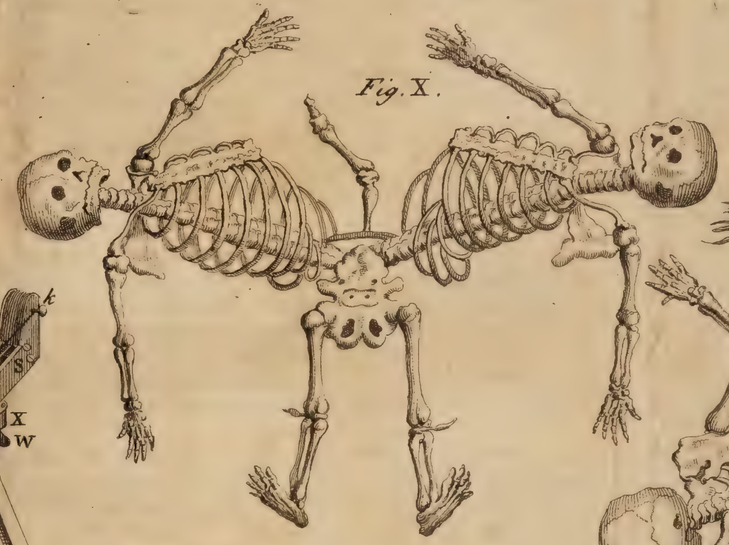


Fig. IX.

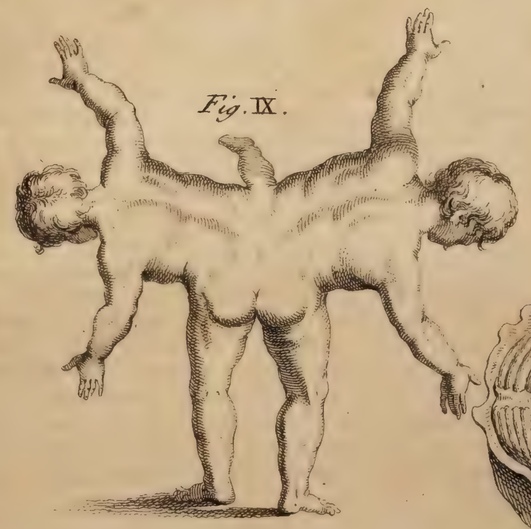
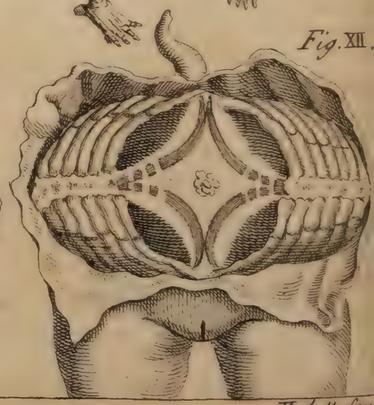


Fig. XIV.

Fig. XI.

Fig. XIII.

Fig. XII.



Wednesday Sep. 5. 1722. From 10 o'clock till ½ an hour after 10.

Wednesday October 3. About 9 o'clock at night.

Thursday October 4. About 10 o'clock at night.

Sunday December 23. 1722. About 8 o'clock at night.

An Account of a reflecting Telescope, made by Mr. Hadley, *together with the Description of a Machine, contriv'd by him for applying it to Use.* Phil. Trans. N° 376. p. 303.

THE instrument consists of a metalline *speculum*, about six inches in diameter: The radius of the sphere, on which its concave surface was ground, is 10 foot, 5 inches and ¼; and consequently its focal length is 62 inches ⅝: The back has a hollow screw made at its center, to receive the end of the handle, which is screw'd on, whenever the metal is to be mov'd, in order to avoid sullying its polish'd surface by handling.

This object metal A (Fig. 1. Plate VIII.) is placed in one end of an octangular tube BB, about six foot long, and something wider than what is sufficient to receive the metal, dyed black on the inside: About six or seven inches in length of the three uppermost sides of the tube C (towards that end, at which the metal is placed) are separated from the rest, and open with two hinges, to make room for the metal to be put in and taken out. The end of the tube is clos'd by an octangular piece of board D, which has an opening *d*, about ⅛ of an inch broad, from the top down to a little below the center, to give room for the above-mentioned handle, when the object metal is lifted into or out of the tube; at other times it is clos'd with a sliding shutter: The metal is placed in such a manner, as to have its axis coincide with that of the tube, by the means of three small buttons, fix'd to the inside of the tube, having their hinder ends all in the same plane, to which this axis is perpendicular: Two of these appear at *a a*, the third, being at the middle of the bottom of the tube, is not seen: The foreside of the metal rests against these buttons in three points of its circumference, nearly equidistant from each other, and is held to them by three screws (one of which appears at *b*) which run thro' the octangular board at the end of the tube and bear against the back of the metal (in three points, which directly answer those three on the foreside)

foreſide) with juſt ſo much force as is requiſite to keep it ſteady in its place; they muſt not be ſcrew'd harder againſt the metal for fear of bending it, which (tho' it is half an inch thick) a very little force is ſufficient to do. When the inſtrument is not us'd, theſe ſcrews are looſened and the object metal is taken out and laid by, to prevent its tarniſhing.

The oval plane is compos'd of a plate of the ſame metal with the great *ſpeculum*, about $\frac{1}{15}$ or $\frac{1}{16}$ part of an inch thick, ſolder'd on the back to another of braſs; its breadth is ſomething leſs than $\frac{1}{2}$ an inch, and is to its length, as 1 to $\sqrt{2}$. At one end of the oval, the braſs plate projects a little beyond the other, and has a ſcrew cut thro' it in that part, as likewiſe another directly againſt the center of the foreſide. The other end is cypher'd away on the backſide, that it may intercept as few of the rays, in their paſſage towards the object metal, as is poſſible. The two ſcrew holes in the back ſerve to fix this oval A Fig. 2. to a braſs arm B, which is faſtened at the other end into a ſlider E E, Fig. 1, 2. This ſlider is of an equal thickneſs with the ſide of the tube, and has a groove G G Fig. 1. cut for it in that ſide, parallel to the axis, and long enough to give room for its motion, to ſet the two *ſpecula* at the different diſtances, which the ſeveral eye-glaſſes require. It reſts on the inſide againſt two thin ledges, faſtened within the tube, along the ſides of the groove. On the outſide it is kept in its place by a ſliding ſhutter, not expreſs'd in the figure. In the middle it has a cylindrical cavity D Fig. 2. whoſe axis is exactly perpendicular to its inner and outer ſurfaces. Each of the boxes, in which the eye-glaſſes are contain'd, is fitted to this cavity; the above-mentioned braſs arm is fix'd into the inſide of this ſlider, towards the end fartheſt from the object metal; it riſes perpendicular for about two inches, and is made flat, ſo as to turn one edge to the rays, which come from the object; about *b* it is bent forwards and flatted the other way; ſo that when the back of the oval plane is held flat to it, by the two ſcrews *c c*, the axis of the cylindrical cavity may fall on the center of its foreſide, inclin'd to its ſurface in an angle of ſomething leſs than 45 degrees: This angle is brought to be exact by two very ſmall ſcrews *i i*, whoſe threads take hold in the flatted end of the braſs arm, and their points bearing againſt the back of the oval, raiſe one end of it a little from the flat of the arm. The *ſpecula* are ſet at their

due

due diſtance, by turning a long ſcrew C C, for which there is a nut, lodged in the ſlider at g; the ſcrew is kept from moving backwards or forwards when it is turn'd, by a braſs plate F, which is to be fix'd to the flat end of the ſide of the tube, and taken off at pleaſure. Each of the eye-glaſs boxes H has a ſcrew on the outer end, to faſten it to a bowl or diſh I, to receive the ball of the eye, and guard it from external light.

On the top of the tube is fix'd on two ſmall pedeſtals, a common dioptric teleſcope, H Fig. 1. about 18 inches long, its axis parallel to that of the tube; and having two hairs, placed in the common *focus* of its object and eye-glaſſes, croſſing each other in its axis.

There are three convex eye-glaſſes, belonging to the inſtrument; the firſt or ſhalloweſt has its focal diſtance of about $\frac{1}{3}$ of an inch; the ſecond of $\frac{1}{10}$, and the deepeſt of $\frac{1}{20}$ or ſomething leſs. When the firſt of theſe is us'd with the inſtrument, it magnifies about 188 or 190 times in diameter; with the ſecond about 208, and with the third 228 or 230. Each of theſe glaſſes has a circle, placed in that *focus* neareſt the oval, to determine the part of the object, ſeen at one view; and in the other *focus* towards the eye, a braſs plate with a little hole in the middle, to let no light paſs to the eye from the inſide of the tube, but what comes from the oval. Beſides theſe three convex, there are two concave eye-glaſſes, with which it magnifies about 200 and 220 times, and alſo a ſet of three convex, which turn it into a day-teleſcope, magnifying about 125 times: The aperture is limited by a circle of card, or paſteboard, placed before the object-metal in the tube. To vary the aperture there are three of theſe circles, and the apertures allow'd by them are five inches and $\frac{1}{2}$, five inches and four inches and $\frac{1}{2}$; tho' for ſome objects the whole metal may be left open.

The engine, made uſe of to direct the tube to any object, conſiſts of a ſtrong plank F F, Fig. 1 and 3, about 14 inches wide and two foot and $\frac{1}{2}$ or three foot long, which ſerves as a foundation for the whole. Near one end of this plank is placed an upright four-ſided box III Fig. 1, 3, about two foot high, narrower at the back next the end of the plank than before: Its two ſides are mortis'd both into the plank below, *a a* Fig. 3. and into the top of the box above, *d d*; the back and forepart are faſtened to the edges of the ſides with wooden ſcrews; the top has a circular hole cut in it, ſomething above three inches in diameter, whoſe center is

about three inches distant from the outside of the back, and at an equal distance from the two sides; this hole gives passage to a turning pillar B, in the bottom of which is fix'd an iron pivot *c*, to turn in a thick brass plate, lodged in the plank *b*. The upper end of the pillar rises about an inch and a half above the top of the box, and is mortis'd into a strong head K Fig. 1, 3. about eight inches in length, and four or five in breadth and thickness. This head carries two cheeks L L, about 13 or 14 inches in height, their hinder edges towards the lower end, extending five inches beyond the axis of the pillar backwards: Along the back of these cheeks, at equal distances above each other, there are notches tending obliquely downwards, and answering one another in each cheek, to receive the pivots of a crooked iron axis C, Fig. 3. on which the tube is placed. The notches are made at different heights, to keep the eye-glass at a proper height for the eye, in different elevations of the object above the horizon. The figure of the axis answers that of the three under sides of the tube. The axis of the tube lies about two inches and a half higher than the axis of the motion upon these pivots; and the center of gravity, when the object-metal is in, is about three inches more backwards. To keep the tube from slipping back, when its fore-end is rais'd, it has two buttons fix'd to it, that rest against the fore-part of the axis.

To keep the pillar from touching any of the sides of the round hole, in which it turns, a cylindrical sector, containing about 65° or 70°, and about an inch in height, is cut out on the back part of the pillar, near the upper end D; in the angle of this cavity, is fix'd a thin steel plate *e e*, bent cross the middle to the same angle; the internal angular edge, between the two parts of this plate, lies in the axis of the pillar, and turns upon the hardened edge of a wedge-like iron *f*, whose base, or board part, is fastened with two strong screws on the top of the box, directly behind the round hole above mentioned. The upper parts of the cheeks are strengthened by two brackets G G, leaving room between them for the bottom of the tube to touch the upper edge of the fore-part of the head; the hinder part of the head is also hollow'd in the manner represented in Fig. 3. The head on its fore-part carries a flat arm M, Fig. 1. about 27 inches long, somewhat tapering towards the farther end, where it is four inches broad: This is strengthened by a narrow slip, glu'd edgeways along the middle underneath O, and also by a brace

or ſtay N, reaching from the turning pillar to within nine inches of the end of the arm. The ſtay paſſes thro' a tranſverſe opening, cut in the fore-part of the box P, which is long enough to allow room for a ſufficient motion of the pillar round its axis.

On the other end of the bottom plank, a-croſs its length, is erected a board Q about 12 inches wide, and 26 or 27 high, the top of it reaching within an inch and an half of the under ſide of the arm: This board is held firm in its poſition by a ſpur R; part of its upper end on the outſide is pared off towards the edges, to form it into the ſegment of a cylinder, whoſe axis coincides with that of the pillar; its uſe is to ſupport a reſt SS, on which the end of the flat arm moves backwards and forwards: This reſt being applied tranſverſely to the outer part of the upright board, where it is made cylindrical, is bent into the ſame figure, by the means of four ſcrew-pins, two of which paſſing thro' each end of this and of another piece of the ſame length, T (but ſomewhat narrower) placed over againſt it on the inſide of the board, by their nuts, draw them together, ſo as to graſp the end of the upright board between them; the upper edge of the reſt being firſt ſhot with a plane, very ſtrait and ſmooth: To render the motion of the arm along the reſt ſmooth and eaſy, it has two rollers, lodged in a box, fix'd near the end, on its under ſide V, to roll upon the edge of the reſt, when the end of the arm is mov'd along it; one of the rollers is placed near each edge of the arm, and their axes lie in lines, paſſing thro' the axis of the turning pillar; the reſt is kept up to them, with a proper degree of force, by two ſcrews W W, which run into two plugs X X, faſtened on the ſides of the upright board, and bear againſt the under ſides of two pieces, fix'd on the inſide of the reſt.

The motion of the tube is directed by two braſs pegs Y and Z; the firſt of theſe Y is placed about 10 or 11 inches from the end of the arm, and has a line wound round it, which paſſing under a ſmall pulley *f*, fix'd in a vertical poſition, near the end of the arm, is faſtened to a ſtaple on the under-ſide of the tube *g*: This line, by the turning of the peg, brings the fore-end of the tube to its due elevation, being acted againſt by the exceſs of weight in the hinder end of the tube, when the metal is in it, which is equivalent to about two pounds at *g*, where the line is faſtened. In great elevations of the object above the horizon, the line is not carried ſo far, as the point *g*; but is faſtened a little above the pulley, to

a light ſquare ſtick *b*, having a hook at one end, by which it takes hold of the ſtaple *g*. This is done, that the ſpringineſs of the line may not continue a vibrating motion in the tube (when any thing happens to ſhake the inſtrument) and make the object appear to tremble; the lower part of the ſtick reſts againſt the end of the arm, and by its ſlight friction contributes to the ſame effect.

The other peg Z is placed in ſuch a manner, that it may be conveniently reach'd by one hand of the obſerver, while the other is employ'd about the peg Y; it regulates the horizontal motion of the tube, by means of a line, which being wound about the peg at one end, paſſes by another ſmall pulley, placed cloſe by the ſide of the above-mentioned one in an horizontal poſition (not to be ſeen in the figure) and is hung on a pin, driven into the little head K; it is acted againſt by two ſprings *m* and *n*, Fig. 3. placed in the box III, one on each ſide of the turning pillar; that on the right hand *m*, draws the right ſide of the pillar forwards, by a very ſtrong line, which, being faſtened to the head of the ſpring, paſſes round the back part of the pillar to a pin at P, by which it is ſtrain'd to its due ſtrength: The ſpring on the left hand *n* draws the left ſide of the pillar backwards in the ſame manner. Theſe pins are placed on the pillar a little higher than the tops of the ſprings, that being drawn a little downwards, as well as turn'd round its axis, the pivot in its bottom may not be rais'd out of the hole in the braſs plate, when the reſt bears hard againſt the rollers at the end of the arm: Each of theſe ſprings draws with a force, equal to about 18 or 20 pounds weight, when the end of the arm is carried cloſe to the ſmall head *k* Fig. 1. and conſequently (the ſemi-diameter of the pillar being 1 inch and $\frac{1}{2}$, and the diſtance of that head from the axis about 28 or 29 inches) the end of the arm will be carried by the united forces of both the ſprings, towards the other end of the reſt, with a force equivalent to the weight of about two pounds; each of the pegs Y and Z turns in a hole, made in a piece of wood *l*, faſtened to the under ſide of the arm; and the pieces being ſlit with a ſaw from one end thro' the hole, and about $\frac{1}{2}$ an inch beyond it, the ſeparated parts are drawn together by a ſcrew *m*; till the end of the peg be griped between them, with a due degree of force: By theſe pegs, with the help of the teleſcope H, the tube is eaſily directed to any object, and made to accompany a celeſtial one in its diurnal motion; while the end of the arm moves the whole length of the reſt.

If

If it be desir'd, that when the object is found, the turning of one peg shall carry the tube along with the motion of the heavens, so as to keep the object always in sight; this may be easily effected in various manners.

The concave surface of the object metal has several little pots in it, which could not be brought to take a polish: In one or two places, the metal itself seems to have some small parts, something harder or softer than the rest, occasioning an irregularity in the figure of the metal about them. But these parts being small, in proportion to the whole, do not seem considerably to affect the distinctness of the appearance.

The open air has commonly an undulating motion in its parts, especially in the day-time, which occasions the rays of light to deflect a little from the streight lines in which they ought to move, in order to render the species perfectly distinct. The effect of this, tho' insensible to the naked eye, or even thro' a small telescope, becomes considerable, when the object is very much magnified. This instrument, when tried at an object inclos'd, so as to secure it from this inconveniency, seems to bear an aperture of 5 inches and $\frac{1}{4}$, with the deepest of the above-mentioned eye-glasses, as well as the common telescopes do the usual charge and aperture given to them, excepting that in these the objects appear a little brighter.

Fig. 1. represents the instrument, placed on the machine in order to be applied to use.

Fig. 2. represents the inside of the slider, with the rest of the *apparatus*, belonging to the oval plane and eye-glass.

Fig. 3. represents the hinder part of the machine; the back and one side of the box being taken away, to shew the turning pillar and springs on the inside.

Fig. 4. represents *Saturn*, as he appear'd in *June* 1720, by this telescope.

The Dissection of a decrepid old Man, 109 Years of Age; by Dr. John James Scheuchzer. Phil. Trans. N° 376. P. 313. *Translated from the* Latin.

THE different stages of human life have their own peculiar characters: For, man gradually ascends from a state of fluidity thro' the various degrees of solidity, till at length he die with a rigidity and immoveable stiffness of the

the fibres, as it were; the conſideration of this is of no mean uſe in phyſick.

John Leonardus Vopper, a *Griſon*, was born *May* 1. 1614. In 1634 being employ'd as a miner, he lay buried in a vein under ground for 33 hours; and being taken out from thence, he ever after, by reaſon of the ſtrong preſſure on the *Abdomen*, was ſubject to incontinency of urine. In 1637 he travell'd thro' *Hungary*, *Turky*, and the *Holy Land* and return'd to *Venice*: In 1639 he ſerv'd in the Duke of *Lorrain*'s army in *Milan*, and in 1662 in *Portugal*: In 1682 he was at the ſiege of *Vienna*, and in the laſt war, at the ſiege of *Landau* and battle of *Hockſtet*. Thus thro' a variety of ſcenes he liv'd till he was 109 years and three months old.

In a curſory view of his body, the following particulars were obſerv'd.

There was a ſmall quantity of bloody extravaſated *ſerum* in the cavity of the *abdomen*.

All the thin guts were inflam'd, and tinged with a red colour; and particularly the *duodenum* was very much dilated, its inſide quite mortified and gangrenous; the *omentum* was entirely emaciated, ſo as hardly to be diſtinguiſhed; the *pancreas* contracted; the liver ſound; the gall-bladder full of bile; as alſo the *ductus choledochus*, and all the parts adjoining to the inteſtines and meſentery tinged green, as if the bile had been extravaſated ſome where, but its exit into the *duodenum* could not be traced: Near the *pylorus* in the upper part of the ſtomach, there was a flatulent expanſion, as it were, bigger than a walnut. The kidneys were ſound; as alſo the ſpleen, whoſe external membrane had here and there bright white ſpecks, ſome broader than others, which at firſt ſight reſembled the ripe puſtules of the ſmall-pox, but entirely of a cartilaginous hardneſs, and ſomewhat rais'd above the reſt of the ſurface. Here Dr. *Scheuchzer* obſerves, that the fibres do not only contract and become rigid in extreme old age, but almoſt inſenſible; for, no young perſon, or even one of more advanced years, could endure an inflammation of the inteſtines, ſuch as this was, without the moſt exquiſite pain; yet this patient did not in the leaſt complain of it: That cartilaginous cruſt on the ſpleen plainly ſhews, that the membranes may become ſo rigid, as at length to become hard.

There

There was a great deal of difficulty in opening the *thorax*: For, not only theſe prominences of the *ſternum*, into which the ribs are inſerted, and which are otherwiſe cartilaginous, were in this ſubject plainly bony, but almoſt form'd one continued body with them; a manifeſt proof that cartilages in time become bony, as membranes and arteries firſt become cartilaginous, and afterwards bony: The lungs in both ſides of the *thorax* had ſeveral green ſpots, and behind they adher'd to the ribs: The *pericardium* was large and fill'd with a great deal of *ſerum*, ſome of which floated in the cavity of the *thorax*: The heart itſelf was pretty large, particularly, the auricles were exceedingly dilated; and both they and the ventricles were full of concreted blood; whence it appears, that the motion of the blood was at length ſtopp'd, for want of the elaſtic contraction of the arteries, and that the machine of the heart was no longer able to throw out the blood, diſpoſed to concretion; nay, that it was overpowered by the too great reſiſtance: That tendon, by which the arteries are inſerted into the heart, was either bony, or cartilaginous at leaſt: It becomes bony, we know, in old deers, whence the bone in the heart of a deer: And ſome ſuch thing was obſerv'd in this ſubject; for, the ſemilunar valves, particularly thoſe of the *aorta* were almoſt cartilaginous, eſpecially in the middle; the deſcending *aorta* was exceeding large, being double the diameter of the gullet, and about a *Paris* inch: The gullet had ſeveral indurated lentiform glands. This ſize of the *aorta* ſhews, that the progreſſive motion of the blood, probably, become ſlower in his latter years; whence the arteries, next the heart, were gradually dilated, as is frequently the caſe, when the reſiſtance of the maſs of blood to be thrown out is too great.

To come next to the head; the ſubſtance of the *cranium* was exceeding hard: The ſutures, particularly the ſagittal and lambdoidal, were almoſt obliterated, nor did they quite penetrate both tables, ſo that there was no perſpiration; and the nutritious juice of the bones, extravaſated between the fiſſures of the ſutures was alſo become bony: It is true, there were three or four pretty large holes in the *vertex* on each ſide the *ſinus*'s of the ſagittal ſuture, about an inch from each other, that penetrated into the internal *lamina*, as far as the external, yet without any of them perforating both tables: The *dura mater* was near thrice its uſual thickneſs, almoſt like a piece of leather; the *pia mater* was eaſily ſeparated by being moiſ-

moistened with too much *serum*, from the substance of the brain: All the ventricles were full of *serum*, and a large quantity in the basis of the brain, especially at the exit of the spinal marrow. The *plexus choroides* was covered with glands of the bignets of a pea, and full of a coagulated *lympha*: The *septum pellucidum* was very distinctly seen: Moreover, the substance of the brain, particularly the internal part, was more flaccid than usual.

The Dr. was informed, after the death of this old man, that in his life time he had told persons of credit, that his father, after having liv'd above a century begot three children; and that few of his ancestors liv'd less than a 100 years: This is certain, that the old man here spoken of, had a daughter baptiz'd at *Diessenhove Aug.* 18. 1707.

An Account of the Coati Mondi *of* Brasil; *by Dr.* George Mackenzie. Phil. Trans. N° 377. p. 317.

THE *Coati Mondi* of *Brasil* is seldom or never brought alive into *Europe*; yet there were two of them found in Capt. *Green*'s ship, a pyrate; one of which died in Dr. *Mackenzie*'s custody of a wound it had receiv'd in the thigh, which he caused to be dissected; an account of which, compared with that, published by the Academists at *Paris*, the Dr. communicated to the *Royal Society*; it differed from theirs in several particulars, most of which might, as the Dr. thinks, proceed from the difference of sex, theirs being a male and his a female.

Theirs was six inches and ½ from the end of the snout to the hinder part of the head; his was only four inches: Theirs was 16 inches from the *occiput* to the beginning of the tail; his was 10: Theirs was 13 inches from the insertion to the end of the tail; his 12: Theirs was 10 inches from the top of the back to the extremity of the fore-feet; his was 7: Theirs was 12 inches from the top of the back to the extremity of the hinder feet; his 8: The snout of theirs was very long and moveable, like that of a hog, but streighter and longer in proportion; his was only 2 inches: The fore paws had each five toes, the claws of which were black, long and hollow, like those of the *Castor*: The toes of the fore paws were somewhat longer than those of the hinder-paws; the soles were without hair; the palms and soles of these fore-paws were covered with a soft and tender skin; the sole of the hinder-paw was long, having a heel, at the extremity of which were several scales a line broad, and five or six lines long; in all which they perfectly agreed.

The

The ears were round, like those of a rat, and covered a-top with very short hairs; and in this they likewise both agreed, as they did in the eyes, which were exceeding small and beautiful; but there was some difference in the hair: For, theirs was short, rough and knotty, blackish on the back and head, and the rest of the body, mixt with black and red; but in his, the hair was long, in proportion to the animal, especially on the tail, and the whole was beautified with white and black circles, which made it have a most lovely aspect. But from the snout down all the throat and belly to the top of the tail and inside of the legs, was of a reddish colour: The tongue both of theirs and his was chopt with several fissures or strokes, which made it rough to the touch. The *incisores* were six in each jaw: The *canini* were very large, especially those of the lower jaw; but they did not turn up like tusks, as theirs did; they were not round, blunt or white, like those of a wolf, dog, or lion, but sharp, by means of three angles, which at the extremity form'd a point like an awl: As to their colour they were greyish and somewhat transparent: The *gula* was large and cleft like a hog's; and the lower jaw, as in a hog, was much shorter than the upper.

By the dissection the Dr. found in this, as the Academists at *Paris* had done in theirs, that under the skin, and between the muscles, there was a great deal of fat, white and hard, like tallow: Theirs, being a male, had a *penis*, provided with a bone, whose length did in proportion very much surpass that of the bones, which are found in the *penis* of other animals; so in his, being a female, he observed, that it had an exceeding large *matrix*, and that the insertion of the *urethra* was upon the right side of the *vagina*: The *epiploon* in his, as in theirs, was very small; it had but little fat, and was a complication of fibres and fillets, rather than a membrane; it did not lie upon the intestines, but touch'd upon the stomach. In theirs they observed a very large spleen, but in his he could discern none. He did not observe, any more than they had done, any vessels in the external membrane of the stomach, but the *coronaria stomachica*, which appeared as in theirs, towards the upper orifice and soon disappeared, shooting forth a few branches. The liver in his, like theirs, was somewhat blackish, and of a substance very homogenous, without any appearance of glands. It had seven lobes, two large ones on the left side, and five small ones on the right. The *pancreas* in his, as in theirs, was fastened along the *duodenum*, inclining more towards the right

kidney than the left: But whereas it was very ſmall in theirs, it was very large in his. The meſentery in his, as in theirs, was filled with a very hard fat, which incloſed and almoſt conceal'd all its veſſels. The inteſtines in theirs were ſeven foot long and all of one thickneſs, having nothing to diſtinguiſh them; but in his they were only 42 inches and $\frac{1}{2}$. Theirs had no *cæcum*; but he found one in his at the upper end of the *rectum*. The bladder was very large; the right kidney in his, as in theirs, was a great deal higher than the left, and covered with the lobes of the liver. The lungs in theirs had five lobes, two on the right ſide and two on the left, and the fifth in the *mediaſtinum*, which was as thin as a cob-web; but in his there were ſeven lobes, three on the right, three on the left and the ſeventh in the middle. The heart in his, as in theirs, reſembled that of a dog, having the right auricle exceeding large; and as they found a great deal of ſlimy matter, hardened in the right ventricle; ſo he found in his a *polypus*. The *muſculus Crotophites*, paſſing under the *zygoma*, was in his, as in theirs, faſtened there, being extraordinary fleſhy, even to its inſertion, made by a very large tendon, which was incloſed between two pieces of fleſh, much thicker than thoſe, which are generally found in this place, and which are thought to be put there to defend and ſtrengthen the tendon of the muſcle of the temples.

The tendons in the articulations of the fore-feet were very large and ſtrong: In his he obſerved two glands on each ſide of the *anus*, with a paſſage to each of them, full of a greyiſh fetid matter. The orbit of the eye in his was not bony throughout like theirs, but ſupplied in the upper part by a cartilaginous ligament, which joined the *apophyſis* of the *os frontis* to that of the firſt bone in the upper jaw. The bone, which ſeparates the *cerebrum* from the *cerebellum* was, as it is in dogs. The *dura mater* in his did not adhere to the *cranium*, as in theirs. The *ſinus*'s of the *os frontis* in his, as in theirs, were full of matter, like a friable fat. The mammillary *proceſſes* in his, as in theirs, were very large. As to the eye, both of them agreed exactly, the globe not exceeding four lines and $\frac{1}{2}$ in diameter, the aperture of the lids being much larger, and the pupil as large as the whole globe of the eye; the cryſtalline humour contained three lines in breadth, and two and $\frac{1}{2}$ in thickneſs, and was more convex internally than externally; this thickneſs of the cryſtalline humour made the two others to be leſs in quantity. The *choroides* was all over of the ſame colour, *viz.* of a very brown red, without any

tapetum,

tapetum, which is hardly ever wanting in the eyes of other animals.

These are all the remarkable differences he could discover between his own and that of the Academists at *Paris*; only the Dr. gives some account as to the manner of its living and its diet, which they had not an opportunity of observing in theirs.

The Dr. believes the Academists were misinformed, when they affirm, that the *coati mondi* carry their tails erect; the tail of this at least trailing on the ground; nor can he think, that they eat their tails; for, there was no part of her she could endure less to be handled than her tail, the least touching of it would make her cry, or rather hiss like a snake; she could endure no manner of cold; for, in the intervals between the times of eating, she was either beneath the bedcloaths, or on a cushion before the fire, with the heat of which she seemed to be extremely well pleased.

Her ordinary food was butter'd eggs, milk and bread, all manner of roasted flesh, but no fish: The Dr. once tried her with a new kill'd partridge, of which she eat most voraciously; and, for several days after, she was very wild and ungovernable, which made him never after try her with raw flesh. He was likewise apt to think, that their ordinary dens or habitations are under ground in sandy banks, like our rabbits: For, when she was brought into the fields, she would dig up the sand with her paws, with incredible swiftness; so that had she not been chain'd, there would be no possibility of recovering her.

An Account of a large Quantity of Stones *voided by Drinking the* Pyrmont Waters; *by Dr.* Abraham Vater. Phil. Transf. N° 377. p. 322. *Translated from the* Latin.

A Nobleman of *Pomerania*, in perfect health (as he himself affirmed) and without any sensible ailment, was perluaded to drink, by way of preservative, the *Pyrmont waters*: After drinking them for some days, when he made water he felt some small stones come away, without any pain: Judging this to be critical, he began to drink the waters in a larger quantity; upon which, in four or five days after, he voided upwards of 40 stones, yet still without any pain: Upon observing this, he continued to drink them chearfully and plentifully, promising himself a great deal of good thereby. But by drinking too large a quantity of the waters,

he had a total ſuppreſſion of urine for three days, till by a catheter a paſſage was opened: He, therefore, by the advice of his phyſicians, abſtained from any farther uſe of the waters, ſuppoſing the danger thus over. But not long after, being on a journey, he had a conſtant deſire of making water, till at length he voided pure blood with the moſt exquiſite pain: This painful voiding of blood continued all the time of the journey: but after his return home, and that he was quiet, it immediately ceaſed and he felt no more pain: But ever after as often as he rode either in a chariot or a horſe-back, his pain returned, and he voided blood with his urine; but when he was quiet, it again ceaſed without the leaſt pain: It is true, it ſometimes happened, that he voided a ſtone or two, but that was rarely and without any pain; becauſe they were much ſmaller than thoſe voided by drinking the waters. The patient had this troubleſome ſymptom upon him for two years, but at length was relieved by the uſe of medicines. And now being apprehenſive of nothing more, and ſeemingly reſtored to perfect health, he forbore the uſe of them: But from that time his ſtrangury daily increaſed, and he had a conſtant deſire to make water, which ſtill continued upon him. His urine, which was voided in a very ſmall quantity, and with very great pain, was thick, from the admixture of a viſcid *pituita*, that immediately ſubſided and reſembled paſte, and could be drawn out into threads; the patient had all the time a good appetite, ſlept pretty well, and felt no pain in any other part.

This caſe, conſidered in all its circumſtances, the Dr thinks, has ſcarce a parallel in medical obſervations. What is moſt remarkable is, that a man, who (as he himſelf affirm'd) was in perfect health, and who never had any ſigns of the ſtone, ſhould, by drinking theſe mineral waters, void pretty large ſtones, in ſuch quantities, and in ſo ſhort a time. This may give no ſmall reaſon to ſuſpect, that theſe ſtones were not diſcharged by the waters, but rather generated from the mineral *crocus*, with which theſe medicinal waters are impregnated. Tho' the very ſhort time in which this happened (ſcarce ſufficient for the concretion of ſo many ſtones) ſeems little to favour this opinion; becauſe voided a few days after the patient began to drink the waters; yet he thinks it not eaſy to determine, what time is requiſite for the production of a human *calculus*: which probably may be very ſhort: And it ſeems leſs agreeable to truth, that theſe

calculi

calculi ſhould lie concealed in the urinary paſſages; ſince this patient never had any difficulty in making water nor the leaſt ſymptom of the ſtone: On the contrary, all the circumſtances of this caſe confirm the concretion to be inſtantaneous: For, the ſtones had no aſperities and angles, as thoſe of the bladder commonly have, from the ſucceſſive coheſion of the ſaline *ſpicula*; but reſembled large and ſmall grains of peaſe, round, ſmooth and even, which ſhews their concretion to be ſudden: Their dirty colour exactly agreed with that ſediment, extracted from boiling the *Pyrmont-waters*: Upon breaking the *calculi*, the ſame variegated *ſtrata* were obſervable in the inſide, as in the *tophi* of the *Caroline-waters*: What is peculiarly remarkable is, that the greateſt part of theſe *calculi* was voided at the time of drinking the waters, but afterwards very rarely, and theſe much ſmaller. Yet this extraordinary effect is not to be imputed to theſe medicinal waters, as if they contributed to the production of the ſtone, but rather to the weakneſs of the *viſcera* and to a bad digeſtion.

Obſervations on Diſſecting the Body of a Perſon, troubled with the Stone; *by Dr.* Perrot Williams. Phil. Tranſ. N° 377. p. 326.

MR. *William Bowen* of *Haverford-weſt*, between 40 and 50 years of age, having been, for about 7 years, ſeverely afflicted with the uſual ſymptoms of the ſtone in the kidneys and bladder, *viz.* bloody urine after exerciſe, ſtrangury, *&c.* died in *May* 1722. Upon opening his body, there were found in the bladder ſix ſmooth oval ſtones, exactly of the ſame figure and nearly of the ſame bigneſs: There were likewiſe three cells in each kidney, the figure of each anſwering that of the ſtones: The *ureters* were ſo preternaturally extended, as very eaſily to admit the largeſt of the ſtones to paſs from the kidney to the bladder. The *viſcera, &c.* appeared in their natural ſtate.

The Diſſection of the Body of a Man, *who Died of the Stone in the* Kidneys; *by Dr.* Hardiſway. Phil. Tranſ. N° 377. p. 327. *Tranſlated from the* Latin.

A Man about 70 years of age, was *October* 19. 1722, ſeiſed with a very violent colic, attended with vomiting and a ſtrangury, without being able to ſtand upright, by reaſon of the exquiſite agony he was in: On the 6th day of his illneſs,

illneſs, after a total ſuppreſſion of urine, he complained of a painful conſtriction about the *hypocondria*, as if bound with ropes, and afterwards of a troubleſome weight in the bladder, as if a large turnep (for, ſo he call'd it) were lodged therein. The ſymptoms growing worſe and worſe, the patient died on the 12th day of his illneſs. Upon opening the body, the Dr. cut the bladder, but found not the leaſt ſign of a ſtone therein; but in both kidneys (which contained a large quantity of water) he found a remarkable ſtone (repreſented Fig. 5 and 6. Pl. VIII.) rough, and ſpreading into a variety of branches thro' the *parenchyma*, and which could not poſſibly be taken out whole, without firſt cutting away the fleſh; the largeſt branch, inſinuating into the head of the *ureter*, ſhut it up entirely: The *omentum* was retracted under the liver, like a narrow belt. The patient was of a very robuſt conſtitution and always healthful, only a few years before, he ſometimes voided gravel with his urine.

Fig. 5. A repreſents the upper part of the ſtone which lay in the left kidney; B its *apex* towards the ribs; C a branch; D the branch, that ſhut up the head of the *ureter*.

Fig. 6. *a* repreſents the *apex* of the ſtone (which lay in the right kidney) regarding the ribs; *b* the upper part; *c* a protuberance; *d* a branch extending towards the *coxa*; *e* a cavity, where an oſſicle was broken off, forming a protuberance or proceſs; *fff* the lower part of the ſtone, that poſſeſſed the head of the *ureter*.

An Account of the Depth of Rain *fallen from* April 1. 1722 *to* April 1. 1723. *obſerved at* Widdrington *in* Northumberland; *by Mr.* Horſley. Phil. Tranſ. N° 377. p. 328.

MR. *Horſley* kept an exact account of what rain fell at *Widdrington* in 1722: Weighing the water and reducing it from weight to depth ſeemed pretty troubleſome, even when done in the eaſieſt method: To remedy this inconvenience (beſides a funnel and proper receptacle for the rain) he made uſe of a cylindrical meaſure and gage. The funnel was 30 inches diameter, and the cylindrical meaſure exactly three inches; the depth of the meaſure 10 inches, and the gage of the ſame length, with each inch, divided into 10 equal parts; or inſtead of a gage, the inches and diviſions may be mark'd on the ſide of the cylindrical meaſure: The *apparatus* is ſimple and plain, and it is eaſy to comprehend the deſign and reaſon of the contrivance. For, the diameter of the cylindrical meaſure being juſt $\frac{1}{10}$ of that of the funnel, and the meaſure exactly 10 inches deep,

deep, it is plain, that 10 measures of rain make an inch in depth; 1 measure, $\frac{1}{10}$; 1 inch on the gage $\frac{1}{100}$, and $\frac{1}{10}$ of an inch on the gage, $\frac{1}{1000}$, *&c.* By this means the depth of any particular quantity, which falls, may be set down with ease and exactness; and the whole at the end of each month or every year may be summ'd up without any trouble.

By the following account it appears, that some of the summer months, particularly *May* and *July*, were very wet, and some of the winter ones very dry; so that one with another, this year's rain, as far as can well be conjectured, may be reckoned as a medium; and if so, it differs not above two or three inches, from the mean quantity of rain which falls at *Upminster*, *Paris* and *Lisle*; being less than that at *Lisle* and more than that at the other two places.

An Account of the depth of rain fallen from *April* 1, 1722 to *April* 1, 1723.

	Inch. Ten.
In *April*	1 , 015
In *May*	3 , 532
In *June*	2 , 570
In *July*	4 , 350
In *August*	2 , 132
In *September*	1 , 155
In *October*	, 600
In *November*	2 , 205
In *December*	1 , 780
In *January*	1 , 225
In *February*	, 485
In *March*	, 195
In the whole year	21 , 244

Of the Blood-globules, *&c. by M.* Leewenhoeck. Phil. Transf. N° 377. p. 341. *Translated from the* Latin.

DR. *Jurin* having discovered a method of determining with certainty the diameters of minute objects; and consequently, that the diameter of a blood globule was equal in bulk to

to the $\frac{1}{1940}$ part of an inch, M. *Leewenhoeck* reaſoned in this manner, if the diameters of 1940 blood-globules be equal in length to 1 inch; moreover, if ſpheres be to each other, as the cubes of their diameters, as Geometricians demonſtrate; it follows that a body, conſiſting of 7,301,384,000 globules, is no bigger than a ſphere, whoſe axis is equal to 1 inch in length.

M. *Leewenhoeck*, upon viewing a-freſh his own blood and that of two other perſons beſides, plainly diſcovered, that four diameters of blood globules were, according to Dr. *Jurin*, equal to the diameter of a fine ſilver wire, which the Dr. had ſent the former; 485 diameters of this ſilver wire were equal to 1 inch or $\frac{1}{12}$ part of an *Engliſh* foot, as found by the method, exhibited in *Phil. Tranſ.* N° 355. M. *Leewenhoeck* only adds, that he could obſerve with his naked eye (ſome tho' very few) blood-globules, four diameters of which ſomewhat exceeded that of the ſilver wire: But he imagined, that theſe globules, drawn from his thumb by pricking it with a needle, and laid upon a glaſs-plate, whilſt they were ſtill warm, had deviated ſomewhat from a round to a plane figure.

Moreover, to this he adds, that beſides his ſight being much impaired by his extreme old age, his right eye was ſomewhat dim; which he ſuppoſes to be owing to a great many blood-globules floating in the cryſtalline humour before his ſight; ſome of which being joined together in no certain order, and others floating a-part, occaſioned a kind of *nubecula* in his eye: For commonly uſing his right eye, when he viewed objects, he could eaſily ſhut his left; hence it ſeemed not to be ſo clear ſighted as uſual.

In *January* 1723, M. *Leewenhoeck* was ſeiſed with a violent motion about the diaphragm, at which the by-ſtanders were very much ſurpriſed: Upon the remitting of that motion, he aſked a phyſician preſent, what he called that diſorder, who told him it was a palpitation of the heart: But M. *Leewenhoeck* believed the phyſician was miſtaken; for tho' during the motion, he himſelf often felt his pulſe, yet he could perceive no acceleration thereof: That violent motion, recurring at times, laſted about three days, at which time his ſtomach and inteſtines loſt their motion, and did not perform their office; upon this M. *Leewenhoeck* verily believed he was near his end.

He

He ſuppoſed, that the obſtruction in his diaphragm, was as big as a *Rix* dollar.

M. *Leewenhoeck* looks upon the common opinion of generation from the *ovarium* as abſurd: For ſeveral years together he examined the *ſemen maſculinum* not only of birds and fiſh, but likewiſe of other animals; and this he chiefly did every year, when fiſh ſpawn: And he commonly obſerved, the *ſemen maſculinum* abounding in a vaſt number of very minute *animalcula*, of which it chiefly conſiſts: Whence he certainly concluded, that theſe *animalcula* gradually become animals of the ſame ſpecies, with thoſe they ſprung from; excepting eels, ſhrimps, ſhell-fiſh and ſeveral ſorts of flies, which are all females and which in this reſpect agree with vegetables; in whoſe ſeeds we find little plants, which inſtead of the *ſemen maſculinum* again produce fruit of the ſame kind with that from which the ſeeds ſprung.

Some Amendments and Additions to the Account of Things found under Ground in Lincolnſhire; *by Mr.* Ralph Thoresby. Phil. Tranſ. N° 377. p. 344.

WHereas, in *Phil. Tranſ.* N° 279. there is ſome difference between the accounts of the depth of the things, found in *Lincolnſhire*; the one reckoning it to be about 8 or 10 foot deep, the other 12 or 14: It is to be noted, that the depth was not meaſured, but conjectur'd and accounted for according to the relator's beſt remembrance. But the difference may be eaſily accommodated or ſalv'd, by ſuppoſing (which will not be far from truth) that when the labourers or dykers firſt diſcover'd the jetties and other things found therein, it might be about the depth of 8 or 10 foot; but the bottom of them, when they came to be all taken up, might be at the depth of about 12 or 14 foot, as in the other relation.

It is alſo to be noted in the ſaid *Tranſaction*, that ſome very judicious perſons affirm, that the ſtones, which the ſpectators ſaw at the bottom of *Hammon-beck*, were ſuch, as the dykers had firſt thrown out (when they were taking up the old *goat*) and were fallen in again: But that it was a hard firm ſoil is certain; and probably, that on which the famous ſteeple of *Boſton* ſtands; as to the foundation of the ſaid ſteeple; vide *Phil. Tranſ.* N° 223.

The form of the ſhoe ſoles, found at *Spalding*, was as repreſented Fig. 7. Plate VIII. each foot had its proper ſhoe; this was for the right foot.

Now, by ſome paſſages in hiſtory, it may be probably conjectured, when thoſe ſhoe ſoles were left there, and how long ſince, that alteration began in that part of *Lincolnſhire*.

In *Stow*'s chronicle *ad An.* 1465 we read of a proclamation againſt the beaks or pikes of ſhoone or boots, that they ſhould not exceed two inches, upon penalties there mentioned: And by other paſſages in hiſtory it appears, that thoſe pikes of ſhoes were before that time exceeding long, and held up by chains, that they might not hinder the wearer's walking; which chains were ſometimes of ſilver, if not of gold, that they might be rich, as well as ornamental.

In *Melchior Adamus*'s life of *Conrad Pellican*, at the bottom of p. 263. *Octavo Edit.* there is a paſſage to this purpoſe, 'at 'that time, *viz.* in 1484, the ſoldiers, who returned from '*Flanders*, introduced ſeveral novelties, to wit, party-colour'd 'ſtockins, ſquare-toed ſhoes, which both men and women be-'fore that time wore piked or ſharp pointed; as alſo a new 'kind of ſquare-toed ſandals, called **Pantoffen**, which ſuper-'ſeded the uſe of wooden ſhoes, called **Holtzſchuh**: In the 'mean time theſe novelties became very faſhionable.'

Now by this account it appears, that it is not much above 200 years, ſince thoſe ſhoes, above deſcribed, were worn; and conſequently, it cannot be much longer, ſince the earth hath been raiſed there to the above-mentioned thickneſs, *viz.* 10 or 12 foot; and ſince *Bicker-haven* grew up to be (as it is at preſent) higher land than the country on each ſide of it, it may be conjectured, what a change a century or two more may make in the out-falls of the rivers of *Witham*, *Welland*, *Nyne* and *Ouſe*; and conſequently, the neceſſity of taking ſome other method for preventing the impending miſchiefs, which threaten the navigation of the ſaid rivers, and thoſe who have eſtates and intereſts in the great level of the fens, and are concerned in the draining of them.

An Account of a monſtrous double Birth *in* Lorrain; *by M.* Fevry. Phil. Tranſ. N° 377. p. 346.

ON the laſt of *December* 1722 M. *Fevry*, one of the Duke of *Lorrain*'s ſurgeons in ordinary, went by his orders to *Domp Remy la Pucelle* to ſee one *Sebaſtiana Camus*, 44 years of age, delivered on the 24th of the ſaid month, about eight o'clock in the evening, of two children, join'd together in the manner, repreſented Fig. 8, 9. Plate VIII. There was one head, one neck, one breaſt, one *abdomen* and two hands on one ſide, and

and as many parts on the other ſide; the whole being well proportioned and plump, joined together by the belly, which was common to both; ſo that one of the heads was in the place, where the other's feet ſhould be, and the other head in its natural place; they had but two legs for both of them, which ſeemed to ariſe from the tranſverſe *apophyſes* of the *vertebræ* of the loins on one ſide; and from the oppoſite region of the loins, came out a leg, ending with a joint, which bended forwards, and at the extremity formed a ſmall ſtump, like a finger, articulated by *ginglymus*. There was but one fundament for both, by which they voided their *fæces*; they had but one navel-ſtring, and the parts proper to the female ſex were alſo ſingle: They eat and drank with their mouths ſeverally, and while the breaſt was given to one, the other cried for it: They ſlept and waked, ſometimes both at the ſame time, and ſometimes, ſeparately. Each of theſe children had been baptiſed; one of them was ſomewhat plumper than the other, which was more puny and not ſo freſh-coloured. The head of the one, which was a little bigger than that of the other, came firſt to the birth, the two arms, lying on the breaſt came next; the legs lay on the ſides of the breaſt of the ſecond; the oppoſite leg, which was ſingle, was extricated afterwards; laſt of all, the arms of the ſecond child, being ranged on the ſide of its head, made it eaſy for the reſt to come out. The bodies of both together were no bigger than that of an ordinary child.

It is obſervable, that the mother could aſſign nothing that had any relation to this event, during the time of her pregnancy.

Jan. 21. 1723. N. S. M. *Fevry* was informed, that both theſe children were equally well in health.

By another account, communicated to the *Royal Society*, theſe children liv'd two months after the birth.

Fig. 8. Repreſents the fore-view of the double child.

Fig. 9. The back-part.

Fig. 10, 11. The ſkeleton.

Fig. 12. The breaſts and abdominal muſcles, after the external integuments were removed.

Fig. 13. Part of the *viſcera* of the *abdomen*.

Fig. 14. The urinary and genital parts.

The two firſt Fig. were drawn in their life time and the five following were taken after their death, by order of the Duke of *Lorrain*.

Obſervations and Experiments on the Sal catharticum amarum, *commonly called the* Epſom Salt; *by Mr.* John Brown. Phil. Tranſ. N° 377. p. 348.

THAT the ſalt, diſtinguiſhed by the name of *Sal catharticum*, is made from what, at the ſalt works, is called *bittern*, is pretty commonly known; but the particular manner how this *bittern* is produced, and from it, theſe ſalts, has not hitherto been communicated to the world, in ſuch a manner, as to become intelligible; and the *opprobrium* unjuſtly caſt upon the ſalt, of its being a counterfeit *Epſom ſalt*, or ſomething made in imitation thereof, or common ſalt diſſolved and re-cryſtalliſed, has very much ſunk its eſteem among the learned in phyſic.

By the account M. *Bolduc* gives in the hiſtory of the *Royal Academy* of *Paris* for the year 1718, he did not ſucceed according to his wiſh in the experiments he tried, to find out what this ſalt was made from, juſtly grounding his reaſons for its not being all made from the *Epſom* waters, or other ſprings that afford bitter purging waters, on the large quantities conſumed and the cheapneſs of its price: After all his curious endeavours, it ſtill remained a ſecret, till he received information from *England* by Dr. *Mendez*, whoſe account tho' very imperfect, is as follows: It comes, ſays he, from *Lemington* and *Portſea*, both in *Hampſhire*, where from heaps of foſſile ſalt, there runs a ſaltiſh, bitter, ſharp and pungent liquor. One would judge by its brackiſhneſs and bitterneſs, that it contained two ſorts of ſalt, the one a ſea-ſalt and the other a bitter ſalt. To ſeparate theſe ſalts, they cauſe this liquor to run thro' hollow drains on the ground; there it gets together and condenſes into ſalt; this they put into a large veſſel, with a large quantity of common water and boil it as long as is ſufficient to diſſolve it; then they let it cool and ſettle for ſeveral days. The water, impregnated with the ſea ſalt, which is the heavieſt, ſinks to the bottom of the veſſel with the earthy parts, and the water impregnated with the bitter ſalt, which is the lighteſt, ſwims a-top. They take off this upper liquor, as long as it retains its bitter taſte, without any pungency; afterwards they boil it in one or two waters, then evaporate it, and it yields white and tranſparent cryſtals, which are the counterfeit *Epſom* ſalt.

Dr. *Seipp* in his deſcription of the *Pyrmont* waters p. 127. ſays, that the common *Engliſh* purging ſalt, which is ſold in great

great quantities in *Germany*, under the name of *Epſom* ſalt, is not prepared from the *Epſom* waters, but is made in *London* from common ſea ſalt and oil of vitriol.

In the ſame page he ſays, that the ſalt, obtained from the *Pyrmont* waters, will part with its own acid ſpirit, upon pouring oil of vitriol thereon, which the *Sal Mirabile* and the *Engliſh* purging ſalt will not. By this means he diſtinguiſhes the firſt ſalt from the two laſt.

Mr. *Quincey* in his *Prælectiones Pharmaceuticæ*, publiſhed ſince his death, ſays, there hath lately been contrived a ſalt from the mineral purging waters, made by evaporation, filtration and cryſtallization: It was firſt called *Sal mirabile* or *Sal catharticum amarum*; but it is now ſo ſcandalouſly counterfeited, that it is little elſe than common ſalt diſſolved and recryſtaliz'd.

Before Mr. *Brown* enters into the account, how this ſalt is made, he firſt ſays ſomething of the genuine ſalt, that has been made from the bitter purging waters, which the learned Dr. *Grew* attempted firſt to make at *Epſom*: Some years after, ſeveral other bitter purging ſprings were found in different counties, and ſalts in ſmall quantities were boiled up from them; but from no place, nor all the places put together, in ſuch large quantities, as from the ſprings on one ſide of *Shooter's-hill* in *Kent*, about the year 1700, which was then in the poſſeſſion of thoſe two ingenious chemiſts Mr. *George* and Mr. *Francis Moult*; and where they made ſuch large *apparatus* for evaporating the water, that they ſometimes boiled down 200 barrels in a week; from which in a dry ſeaſon, and when the land floods did not get into their drains, they obtained 224 pounds of ſalt.

After theſe works had gone on ſome time; Dr. *Hoy* found out a more expeditious way of making a purging ſalt, ſo nearly reſembling that from the purging ſprings, in all its properties, that it ſoon paſſed on the world for the other, and continued ſo to do.

The great conſumption of theſe ſalts (which then went only by the name of *Epſom* ſalts) both at home and abroad, engaged ſome of our phyſicians (ſeveral years before M. *Bolduc* took notice of it) to ſuſpect, that even what was made at *Shooters-hill* was ſpurious and receiv'd an addition of ſomething to increaſe the quantity: But theſe ſuſpicions, Mr. *Brown* poſitively affirms, were entirely groundleſs, as to the ſalts made there; tho' he readily believes the ſame of any

other

other place, where the ſpring waters were boil'd down for ſalt: But upon a conſideration, that there were greater quantities of this ſalt, conſum'd than all the places, where the waters were boil'd could produce (which was the real fact at that time of day) there was ſufficient room to ſuſpect, that ſome of them were not genuine, as appear'd to be true ſometime after.

For, the ſecret (which was then in a few hands) of making theſe ſalts cheap, gave thoſe who had it, an opportunity of underſelling thoſe who made it from the waters; and in a year or two render'd the latter incapable of making it to any advantage, conſidering the price it was ſold for by the former: So that the work on *Shooters-hill* was thrown up; and Mr. *Brown* believes, there has not been 100 pounds of ſalt made from the waters ſince that time, in any part of the kingdom.

Some time before this work at *Shooters-hill* was broke up, ſome pains was taken to diſcover the ſecret thoſe had, who ſold the ſalt ſo cheap; and upon examining the ſeveral ſalts, that were ſold about *London*, thoſe diſpos'd of by Mr. *George* and Mr. *Francis Moult* were certainly genuine, and were, therefore, a proper ſtandard to judge the reſt by. But from all the experiments made at that time, there could be no material difference found between the ſalt made from the waters, and that made by them who were in the ſecret: There was, it is true, a ſalt ſold by ſome, which, in the courſe of theſe trials, was found to be a *Sal-mirabile*, made from oil of vitriol and common ſalt, but ſhot into ſuch ſmall cryſtals, as not at firſt ſight to be diſtinguiſh'd from the other.

It was not long before this was diſcover'd, and the experiment was tried at the Lady *Carrington*'s ſalt works at *Portſmouth*; where it was found the ſame thing could be done, as at another work, not far from it, and in which Dr. *Hoy* had been concern'd.

It was ſome years after this ſalt had been made at *Portſmouth*, before the ſalt-makers at *Lemington* attempted, or indeed knew the method of making it; who now are the greateſt traders in it, and have ſent ſeveral tons in a year to *London*, beſides what has been directly exported from thence.

It was the opinion of the proprietors of the ſalterns near *Portſmouth*, that this purging ſalt could not be made at any

other

other ſalt works but theirs, and that the bitter taſte in the ſalt was communicated from the earth to the ſea ſalt, whilſt it ſtood expos'd in their ſun-pans. But time has prov'd this opinion falſe; for, beſides what has been ſaid of its being made at *Lemington*, it was, about four or five years ago, *viz.* 1719 or 1720, begun to be made near *Newcaſtle*, where it is ſtill continued to be made; and doubtleſs may be made at any other ſalt-works, where the common ſalt is made from ſea-water by evaporation. Whether any thing of this kind has been attempted at any of our inland ſalt ſprings, either in *Cheſhire* or *Worceſterſhire*, Mr. *Brown* had not hitherto been ſatisfied.

There is ſome difference in the making the common ſalt in *Hampſhire*, from that about *Newcaſtle*: At the firſt of theſe places, in the beginning of the ſummer, at ſpring tides, or at new and full moon, the ſea-water is let into their feeding-ponds, which are their reſervoirs for their ſummer's working, and from hence is convey'd into ſmall ſquare pans; and ſome time after, it is again convey'd from theſe into larger pans or beds, which they call brine or ſun-pans; all which are made of ſea mud and the earth: In theſe laſt pans or beds, it lies expos'd to the ſun and wind, in order to exhale the weakeſt waters; and it is in theſe beds, if the weather prove very favourable, that they can make as good bay ſalt, as any we have from *France*, and at ſuch a time they never bring their brine to the boilers. But if the weather be not hot enough for that purpoſe, their brine is expos'd ſo long in theſe pans, till it become of ſuch a ſtrength, as to bear up their eggs, made of glaſs or wax, to a certain height above the ſurface of the brine; which from thence is convey'd into large ſtore ciſterns, and then into boiling pans, made of iron, where it is boil'd down (after having been frequently ſcumm'd) to the ſea ſalt. It is obſervable, that whilſt the brine is boiling, there precipitates a hard cruſty matter, which is partly taken out by veſſels, placed in proper parts of the pan for that purpoſe, and part of it fixes on the bottom of the iron pan ſo hard, as to be afterwards dug up; and this the workmen call *ſcratch*, and is what Dr. *Collins* in a former *Tranſaction*, concerning the ſea-water, boil'd at *Shields*, calls a ſtone powder. When the operation for the ſea-ſalt is finiſh'd, it is taken out hot, and put into wooden troughs with holes at the bottom, thro' which runs the ſuperfluous liquor: Under theſe troughs other veſſels are ſet (with ſticks fix'd in them

in

in a perpendicular poſition) to receive what runs thro'. The liquor is ſuffer'd to continue ſome time in theſe veſſels, and according to the quantity of the ſea-ſalt ſtill left therein, will cryſtallize to the ſticks, ſomewhat like ſugar-candy, but in much larger ſhoots; and this they call *cat-ſalts* or *ſalt-cats*, and it contains ſome ſhare of bitter ſalt. When this ſalt is broke ſmall, or rather powdered, it is ſo white, that ſome gentlemen chooſe it for their tables; but the greateſt conſumption of it is among the cake ſoap-boilers. The liquor that will not ſhoot to theſe ſticks, is, what at theſe works they call the *bittern*, fit for making the *Sal catharticum*.

Near *Newcaſtle* their method is, to receive the ſea-water into their reſervoirs at high water, at any time of the moon, if there be no freſh water in the river, occaſioned by rain in the higher country; and from theſe reſervoirs, without expoſing of it in beds, as at *Lemington*, they pump it into their boiling pans, where, evaporating it almoſt to a pellicle, they fill it up again eight or nine times, and then evaporate it with a gentle heat for the common or ſea ſalt. The liquor that runs from this ſalt, when taken out and put into proper veſſels, is what they call the *bittern*, from which, if it ſtands ſome time in thoſe veſſels, a ſalt will ſhoot and cryſtallize to the ſides, in taſte pretty much like ſea-ſalt, but with ſome bitterneſs and ſeems to anſwer the *cat-ſalt* of the *Lemington* works, and, very probably, would ſhoot after the ſame manner, if they made uſe of the ſame *apparatus*.

Mr. *Brown* has given this general and looſe account of making the common ſalt, as neceſſary to introduce the liquor, call'd *bittern*; which, before Dr. *Hoy* found out an uſe for it, was always flung away; being ſo different in its properties from the brine made uſe of to produce the ſea-ſalt, that it would not boil up into a ſea-ſalt again, and requir'd the niceſt ſkill and attendance of the operator to determine the time, when to take out the ſea-ſalt from the pans, before the bittern incorporated therewith, which would otherways ſpoil the whole making.

The *bittern* at *Lemington* (as obſerv'd above) not ſhooting to the ſticks, is convey'd by channels into pits, made tight with clay, where it ſtands for ſome months, and there it will ſhoot again: What liquor remains is boil'd down, till ſuch time as it is obſerv'd to be in a diſpoſition to cryſtallize, and then it is convey'd into wooden coolers, lin'd with lead: The liquor, which will not ſhoot there, is boil'd down after the

ſame

ſame manner, in order for another cryſtalization. By this time the liquor ſeems to have alter'd its property, and becomes of a very pungent taſte, and if boil'd down, will no longer ſhoot into cryſtals as before, but precipitate, during the boiling, a ſmall grain'd ſalt; and if, for experiment ſake, you ſhould continue to boil down the liquor, ſeparated from this ſalt, each quantity of ſalt thus produced, will ſtill be more pungent than the other. If you boil down the whole quantity of this liquor, it will produce a ſalt, which, if expos'd to the air, will run *per deliquium*. The liquor, that produces this ſalt, is always flung away, wherever the *Sal-catharticum* is made.

This Mr. *Brown* can give no other name to than a third ſalt, produced from the ſea-water, differing in ſome reſpects, as much from the other two, as they do from each other.

To return to the ſeveral cryſtalizations, ſaid to be ſhot from the *bittern*; theſe will be of different ſizes, as to their figures, and contain ſome ſhare of the third ſalt juſt now mentioned, which makes them apt to give and diſſolve; nor is their taſte come yet to that ſimple bitter of the pure ſalt: Theſe, therefore, are either ſeparately or altogether, to be flung into a copper, with as much common water, as is ſufficient to diſſolve them, and allow of a gentle evaporation, till they are again ready to be pour'd into coolers, in order for cryſtalization. This generally proves to be the pure *Sal-catharticum*, througly freed (as far as the experiments he tried could convince him) either from a ſea ſalt, or the above mentioned third ſalt. The liquor, decanted from this ſhooting, may be boil'd down again, in order for a ſecond ſhooting, and after that a third: But as the liquors from theſe ſhootings are boil'd away more or leſs; ſo you will ſooner or later meet again with the pungent liquor, which contains the third ſalt, as you did in the former ſhootings from the *bittern*, from which the pure *Sal-catharticum* is as neceſſarily required to be freed, as from the common ſalt; a proof of which cannot be better determined, than by one of the experiments hereafter taken notice of, *viz.* that with the oil of vitriol, which will certainly ferment with this ſalt, if the ſea ſalt have not been well ſeparated therefrom, or if it ſtill contain ſome of the third ſalt. And when any of the cryſtalizations will not ſtand the teſt of this experiment, they ought to be diſſolv'd and ſhot again, as before; by which means the pure ſalt is to be obtain'd. This Mr. *Brown* does not mention, as a

trial made uſe of at the ſalt works; but what he himſelf had by experience found to be true; and the ſame experiment will ſerve to diſtinguiſh a *Sal-mirabile*, made at theſe works, from that made with the oil of vitriol and common ſalt: The account they give of it is, as follows; they take any quantity of coarſer grain'd cryſtals, boil'd from the *bittern*, which, when diſſolv'd and evaporated, more than they would otherways do for making the *Sal-cartharticum*, they throw into a wooden bowl, with ſome oil of vitriol, where it ſtands for ten days, and ſhoots into large tranſparent cryſtals, like the *Sal-mirabile*: But as this ſalt, by this method, is not ſufficiently ſaturated with the oil of vitriol (if they uſe any) ſo it is eaſily diſcovered by the oil of vitriol, which will readily ferment with it; whereas, it has no effect on the other *Sal-mirabile*, made as above.

Mr. *Brown* having receiv'd from *Newcaſtle* the ſeveral ſhootings of ſalts from their *bittern*, as alſo ſome of the *bittern* itſelf; from each of them he obtain'd a pure *Sal-catharticum*, as alſo the like kind of third ſalt, as that from the *Lemington* bittern. The method he took in doing it is agreeable to that already mentioned, and ſeveral years before tried at the ſalt works near *Portſmouth*. Mr. *Brown* was inform'd by Mr. *Cay*, that they ſometimes boil'd the *bittern*, without letting it ſtand any time to ſhoot of itſelf: The difference is not very material.

If this account be intelligible, the *Sal catharticum* will no longer be a myſtery: And the next thing worth enquiring into is, whether this ſalt deſerve the reflections, that have diſcourag'd the preſcribing it? And why it may not paſs for a ſalt, as excellent in its kind, and be of the ſame nature and have the ſame properties, as that produced from the *Epſom*, or any other bitter purging ſprings.

And in order to prove it to be ſo, Mr. *Brown* gives a very ſhort abſtract of what Dr. *Grew* ſays of his ſalt, and then obſerves, how nearly the two accounts agree.

The Dr. in his treatiſe *de Natura Salis cathartici amari*, chap. 2, affirms, that in the evaporation of any of the bitter purging waters, they yield a *cremor*, as alſo a ſediment, both together weighing 6,8 or about 10 drachms, from a gallon of water; and that the leſſer part of this ſediment is in ſubſtance the ſame with this *cremor*; the reſt is all ſalt, but conſiſts of two ſorts, one a muriatic ſalt, the other proper or peculiar to thoſe waters.

In

In the *Epſom* water, the muriatic ſalt is about a 20th part of the ſaline mixture; in the *Dulwich* it is in a greater proportion, and the ſame in ſeveral others; it is, both in its acrimonious taſte and figure of its cryſtals, not unlike common ſalt; the other ſalt is what the Dr. ſays is peculiar to the purging waters, and is made by evaporation and cryſtalization. In this preparation, firſt the earthy or plaiſtery part is to be ſeparated, next the muriatic ſalt, and laſtly, a brown and dark liquor from the proper ſalt of the waters.

In chap. 4 of the ſame part, having ſhewn the difference of the figures between the cryſtals of this ſalt and thoſe of alum, the Dr. goes on; neither is there any better ground to account the purging ſalt a ſpecies of common ſalt, from which being entirely freed, it differs as much in taſte, as from alum. And in the ſame chapter he ſays it will appear, the bitter purging ſalt, tho' it hath ſome qualities in common with other ſalts; yet is truly or ſpecifically different from them all: Thus far Dr. *Grew*.

Now, Mr. *Brown* does not ſee any thing in this account, but what will, *conſideratis conſiderandis*, very well agree with the purging ſalt from the ſea-water.

For, firſt, there is an earthy or plaiſtery part, contain'd in theſe waters, and this muſt be ſeparated: The very ſame is in the ſea water, and is precipitated in the boiling them down, as has been obſerv'd, and is call'd *ſcratch* by the workmen.

Next there is a muriatic ſalt, allow'd to be in theſe waters; in ſome more, in ſome leſs, and this is likewiſe to be ſeparated: The very ſame is done from the ſea water, tho' in a vaſtly greater proportion.

And laſtly, there is a black and dark liquor to be ſeparated; tho' this be but a dark way which the Dr. makes uſe of to expreſs himſelf, it cannot be better explain'd, than by what has been found to be fact in boiling down the waters at *Shooters-hill*; *viz.* that after ſeveral ſhootings of ſalt, by repeating the boiling of the waters, there would, at laſt, remain a liquor of a deep brown colour, which would no longer yield a cryſtaliz'd ſalt; but if boil'd up dry, would afford a ſalt of the ſame kind with the third ſalt already mentioned: And this, explaining Dr. *Grew*'s black and dark liquor, helps at the ſame time to prove, in this article too, that the ſea water yields the ſame kind of third ſalt.

Mr. *Brown* tried ſeveral of the Dr's experiments, by which he diſtinguiſhes his ſalt from other ſalts; ſuch as its not affecting the colour of ſyrup of violets; curdling of milk when boil'd; in the figure of its cryſtals; in its eaſy ſolution in the ſame quantity of water; in its coagulating with the oil of tartar *per deliquium*; in its calcination, and in the bitterneſs of its taſte as well before as after calcination, *&c.* and found this ſalt, thus ſeparated from the ſea water, anſwer all the trials. Mr. *Brown* here ſubjoins ſome few experiments, the Dr. had not taken notice of, and then leaves the whole to others to determine, whether there be any ſpecifical difference between theſe two ſalts?

In order to have a ſtandard for theſe experiments, Mr. *Brown* got Mr. *Hyet*, apothecary at *Epſom* (whoſe fidelity he could depend on) to boil down ſome of their waters, which he did from the well in the town, and ſent him a ſufficient quantity of the ſalts, to anſwer the purpoſe he wanted them for.

Mr. *Brown* likewiſe procur'd ſome of the firſt ſalts from the *Lemington bittern*: Theſe do not contain ſo much of what he has already diſtinguiſh'd by the name of the third ſalt, as he found the *Newcaſtle* ſalts do. This *Lemington* ſalt, he, for diſtinction ſake, calls the firſt *Lemington* ſalt.

Part of this he diſſolv'd, and ſhot into pure *Sal-cathartic-cum*; being freed both from the ſea ſalt and the third ſalt; and this he calls the ſecond *Lemington* ſalt.

He likewiſe procur'd from *Newcaſtle* the firſt ſalts, ſhot from their *bittern*, which he calls the firſt *Newcaſtle* ſalt.

Part of theſe he likewiſe diſſolv'd and ſhot, and obtain'd a pure *Sal-catharticum*, and this is what he calls the ſecond *Newcaſtle* ſalt.

He was oblig'd to make uſe of the *Sal-mirabile*, made from the oil of vitriol and common ſalt, that having been taken for the *Sal-catharticum*.

As alſo common ſalt, that having been repreſented, as the principal ſubſtance of the *Sal catharticum*.

He took half an ounce of each of theſe ſalts, and diſſolv'd them in about two ounces of water to each half ounce of ſalt.

A ſmall quantity of each ſolution he poured into as many glaſſes, and drop'd into them all ſome butter of antimony: The precipitation, that followed, ſeem'd to be alike in them all; and upon dropping a little oil of vitriol into each, what

was

was precipitated being more powerfully attracted by the oil, the ſeveral liquors became clear: Theſe are the two only experiments, in which he found the conſequences ſo much alike in them all.

In the following experiments the *Sal-mirabile* is ſufficiently diſtinguiſh'd from all the reſt.

Slices of galls put into theſe ſeveral ſolutions had no manner of effect upon any, excepting that of the *Sal-mirabile*, which is ſoon tinged of the colour of ſack, or rather deeper.

Sp. Sal. Arm. c. Tart. dropped into the ſeveral ſolutions, turns them all milky, excepting that of the *Sal-mirabile*, which keeps its tranſparency.

The *Sp. Salis armoniac c. cale*, the oil of tartar *per deliquium*, the tincture of cochinel in ſpirits of wine, do, each of them, us'd after the ſame manner, ſufficiently diſtinguiſh the *Sal-mirabile* from all the reſt.

In the following experiments, the *Epſom* ſalt, the ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt, agree together and differ from the common ſalt, the firſt *Lemington* ſalt and firſt *Newcaſtle* ſalt.

In the ſeveral ſolutions he dropp'd a ſolution of ſilver in *Aqua-fortis*, from which follow'd theſe conſequences; *viz.* the ſolution of the *Epſom* ſalt, ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt, became equally milky, before the precipitation. The ſolution of the ſea-ſalt and firſt *Newcaſtle* ſalt, let the precipitation paſs, without receiving any milky tincture: The firſt *Lemington* ſalt, as containing leſs of the third ſalt, than the firſt *Newcaſtle* ſalt did, took a little milky tincture. The precipitation fell nimbly thro' the ſolution of the *Sal-mirabile*, leaving it milky.

In the condition theſe were in, he pour'd ſome oil of tartar *per deliquium* to each of them; upon which, after ſome time, a blueiſh ſcum aroſe on the ſurfaces of the *Epſom* ſalt, ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt; There likewiſe appear'd a little on the firſt *Lemington* ſalt; but not any on the reſt.

A ſolution of corroſive ſublimate was made in water, 10 drops of which, mix'd with the ſeveral ſolutions, produced little or no alteration; but upon dropping in the oil of tartar *per deliquium*, the following appearances were produced: In the ſolution of the *Epſom* ſalt, ſecond *Lemington* ſalt

ſalt and ſecond *Newcaſtle* ſalt, the precipitations were red. In the ſolution of the common ſalt, and firſt *Newcaſtle* ſalt, the precipitations were white: In the ſolution of the firſt *Lemington* ſalt, the particles precipitated, approach'd pretty near the colour of the three firſt.

He took ſome of theſe ſeveral ſalts in ſubſtance, and upon each of them he pour'd a little oil of vitriol, which is one of the experiments Dr. *Grew* tried upon his ſalt, and which he ſays cauſes a moderate ebullition, whereby it appears to partake of an alkaline principle. But without looking for this alkaline principle from its fermenting with an acid (terms juſtly exploded by the learned Dr. *Friend* in his *Prælectiones Chem.*) Mr. *Brown* is apt to think, that the ſalt he tried the experiment on, had not, according to his own directions, been thoroughly ſeparated from his muriatic ſalt: For, this oil pour'd on the *Epſom* ſalt, ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt, produced no ſenſible fermentation: It acts with violence on the ſea ſalt, forcing off its acid ſpirit with an intolerable *gâs*. It had the ſame effect in proportion on the firſt *Lemington* ſalt and firſt *Newcaſtle* ſalt; none at all on the *Sal-mirabile*, as being a ſea ſalt already ſaturated with the oil.

What Mr. *Brown* has all along call'd the third ſalt, anſwers in moſt of theſe experiments to the ſea ſalt, and yet has ſome properties exceedingly different therefrom. To thoſe he has already mentioned may be added the following ones; it will not decrepitate like ſea ſalt; it readily melts, when put in a crucible in the fire; and when calcin'd till red-hot, affords a calx equal to, if not ſtronger than a lime-ſtone, and ferments violently, as well with water as with oil of vitriol. This calx, when expos'd to a moiſt air, will part of it run *per deliquium*, but not ſo ſoon as before calcination. All theſe properties differ in every reſpect from the common ſalt; which left Mr. *Brown* ſtill in doubt what to call it; as alſo how far experiments of this kind may be deemed concluſive.

An

An Account of Obſervations made with Mr. Hadley's *reflecting Teleſcope; by Mr.* James Pound. Phil. Tranſ. N° 378. p. 382.

IT were to be wiſh'd, that together with the particular deſcription, given in Phil. Tranſ. N° 376, of the curious mechaniſm of that catadioptric teleſcope, made by Mr. *Hadley*, and by him preſented to the Royal Society, a full account of what obſervations he had made therewith, had been communicated, whereby the public might at length have been appris'd of the uſefulneſs of an invention (worthy of its great author Sir *Iſaac Newton*) which, perhaps, from the vain attempts, made by ſome, of putting it in practice, hath lain neglected theſe 50 years: For, it is ſo long ſince it was firſt publiſh'd in Phil. Tranſ. N° 81.

Mr. *Hadley* has ſufficiently convinced us, that this noble invention does not conſiſt in bare theory; and it is to be hop'd, that he, or ſome other ſuch curious perſons, will in a ſhort time find out a method, either of preſerving the concave metal from tarniſhing, or of cleaning it eaſily, when tarniſh'd, or elſe of making a good concave *ſpeculum* of glaſs, quick-ſilver'd on the back part. When a method for either of theſe ſhall be diſcovered, it is not doubted but that the old dioptric teleſcope will for the moſt part be laid aſide, and this catoptric one be chiefly in uſe among practical aſtronomers; inaſmuch as ſeveral inconveniencies and difficulties, unavoidable in the management of the former, eſpecially when long, are in this latter entirely avoided.

It is no ſmall conveniency, that by means of theſe reflecting teleſcopes, whoſe length exceeds not five feet (and which may be managed at a window within the houſe) celeſtial objects appear as much magnified and as diſtinct, as they do thro' the common teleſcope of more than 100 foot in length.

Mr. *Bradley*, *Savilian* Profeſſor of Aſtronomy, and Mr. *Pound*, having compar'd Mr. *Hadley*'s teleſcope (in which the focal length of the object metal is not quite five feet and $\frac{1}{4}$) with the *Huygenian* teleſcope, the focal length of whoſe object-glaſs is 123 feet, found, that the former would bear ſuch a charge, as to make it magnify the object as many times as the latter with its due charge; and that it repreſents objects as diſtinct, tho' not altogether ſo clear and bright; which may be occaſioned partly from the difference of their apertures (that of the *Huygenian* teleſcope being ſomewhat the larger) and

and partly from ſeveral little ſpots in the concave ſurface of the object metal, which did not admit of a good poliſh.

Notwithſtanding this difference in the brightneſs of the objects, they could ſee with this reflecting teleſcope whatever they had hitherto diſcovered by the *Huygenian*; particularly the tranſits of *Jupiter*'s ſatellites and their ſhadows over that planet's diſk; the black liſt in *Saturn*'s ring and the edge of *Saturn*'s ſhadow, caſt on his ring, as repreſented Fig. 4. Plate VIII.

They alſo obſerved with it ſeveral times the five ſatellites of *Saturn*; in viewing of which this teleſcope had the advantage of the *Huygenian*, at that time when they compared them: For, it being in ſummer, and the *Huygenian* teleſcope being managed without a tube, the twilight prevented them from ſeeing in this ſome of thoſe ſmall objects, which at the ſame time they could diſcern with the reflecting teleſcope.

Obſervations on the Satellites *of* Jupiter *and* Saturn, *with the ſame Teleſcope; by Mr.* John Hadley. Phil. Tranſ. N° 378. p. 385.

MR. *Hadley* gave the *Royal Society* an account of ſome of the moſt remarkable obſervations, he had made with his reflecting teleſcope, before he preſented it to them.

In obſerving *Jupiter*'s ſatellites he had ſeen diſtinctly the ſhadows of the firſt and third, caſt upon the body of the planet: Mr. *Folkes* and Dr. *Jurin*, being preſent, affirm'd, that Mr. *Hadley* had likewiſe ſhewn them the ſhadow of the third ſatellite thro' the ſame teleſcope.

In obſerving *Saturn* in ſpring 1723, at a time when that planet was about 15 days paſt the oppoſition, he ſaw the ſhadow of the planet, caſt upon the ring, and plainly diſcerned the ring diſtinguiſhed into two parts, by a dark line, concentric to the circumference of the ring: The outer or upper part of the ring ſeemed to be narrower than the lower or inner part, next the body of the planet, and the dark line, which ſeparated them, was ſtronger next the body, and fainter on the outer part towards the upper edge of the ring. Within the ring he diſcerned two belts, one of which croſs'd *Saturn*, cloſe to its inner edge, and ſeemed like the ſhadow of the ring upon the body of *Saturn*; but when he conſidered the ſituation of the ſun, in reſpect to the ring and *Saturn*, he found that belt could not ariſe from ſuch a cauſe.

He

He affirmed that at times he had obſerved with this teleſcope three different ſatellites of *Saturn*; but could never have the good fortune to ſee all the five.

Auguſt 1723 Mr. *Hadley* adds, that he ſeveral times obſerv'd the ſhadow of the firſt, ſecond and third ſatellites of *Jupiter* paſs over the body of that planet; and that he had ſeen the firſt and ſecond appear, like a bright ſpot upon the body of *Jupiter*, and that he could keep ſight of them there for about a quarter of an hour, from the time of their entering on his limb.

Jupiter's ſatellites had of late years been ſo ſituated, with regard to the earth and *Jupiter*, that Mr. *Hadley* had not had ſufficient opportunity of obſerving the tranſit of the fourth ſatellite or of its ſhadow.

The dark line on the ring of *Saturn*, parallell to its circumference, is chiefly viſible on the *anſæ*, or extremities of the elliptic figure, in which the ring appears; but ſeveral times he could trace it very near, if not quite round; particularly in *May* 1722, he could diſcern it without the northern limb of *Saturn*, in that part of the ring that appeared beyond the globe of the planet. The globe of *Saturn* (at leaſt towards its limb) reflects leſs light than the inner part of the ring, and he had ſometimes diſtinguiſhed it from the ring by the difference of colour.

The duſky line, which in 1720 he obſerved to accompany the inner edge of the ring croſs the diſk, continued cloſe to the ſame, tho' the breadth of the ellipſis was conſiderably increaſed ſince that time.

An Account of an extra-uterine Fœtus, *that had continued five Years and* ½ *in the Body; by Dr.* Houſtoun. Phil. Tranſ. N° 378. p. 387.

IN *Auguſt* 1717 Dr. *Houſtoun* was ſent for to a woman near *Newport-market* in *London*, who had been married 18 years to a native of the *Eaſt-Indies*, by whom ſhe had eight children, beſides two miſcarriages: At this time ſhe was with child by a ſecond husband, a vigorous young man.

She was near her full time and felt her pains for ſeveral days, which returning by intervals, ſhe concluded, would, as uſual, bring on her delivery. Her mother and the midwife, apprehending no difficulty, aſſured thoſe about her, that only time was wanting.

But the Dr. found upon examination, that her *uterus* was of no bulk to contain a child near its time; and that its neck, of an uncommon hardneſs, was alſo cloſed up ſo ſtraitly, as to give not the leaſt admiſſion, even to a ſmall probe or knitting needle.

Upon this the Dr. affirm'd, that her delivery was impoſſible, becauſe the child was not within the *uterus*, but between it and the guts; that it might be extracted by a paſſage to be made for it, without any great pain and with ſafety to the mother. The Dr. offered to undertake it, and aſſured them this was the only opportunity of giving her any relief.

However the Dr's aſſiſtance was rejected. At that time, probably, it would have been ſucceſsful; for, ſhe was a ſlender well ſhaped woman, in good habit of body and of a ſprightly diſpoſition.

A year after, the Dr. was deſired to viſit her again: Where he found her much diſordered with a growing impoſthumation in her belly: He ordered her ſome cordial ſtomachics, *caſſia* and ſuch gentle lenitives; which met with ſucceſs beyond expectation: So that, by means of a regular diet and the watchful care of a very tender mother, ſhe was reſtored to ſuch ſtrength, that ſhe went chearfully abroad and re-applied herſelf to buſineſs.

About 15 months from the time the Dr. viſited her firſt, her mother came to him to intreat his aſſiſtance; ſhe complained of great pain in the lower part of the *abdomen*, and he found a tumour of a conical figure, projecting about an inch beneath the navel: Its inflammation, with tenſion and feveriſhneſs attending it, ſo plainly indicated ſuppuratives, that he was not ſurpriſed to hear, in a few days, that it had broke, as he had wiſh'd.

The Dr. propoſed to lay it open, both to give a free exit, and prevent its becoming fiſtulous; but the patient was apprehenſive, that he would, as ſhe call'd it, cut open her belly: So that not being able to prevail with her, he ordered her a pot of unguent and ſome plaiſters.

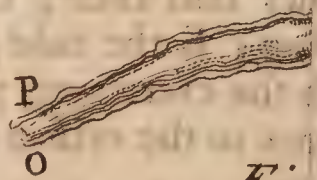
The ulcer ſoon grew fiſtulous, and ſo continued till ſhe died, which was on the 23. of *March* 1723. in the 41ſt year of her age.

For above five months before her death, ſhe voided her excrements by this vent, and all the ſoft parts of the *fœtus*, with ſome ſmall bones of its fingers: But the reſt of the ſkeleton remaining entire, he extracted out of her body, together with the

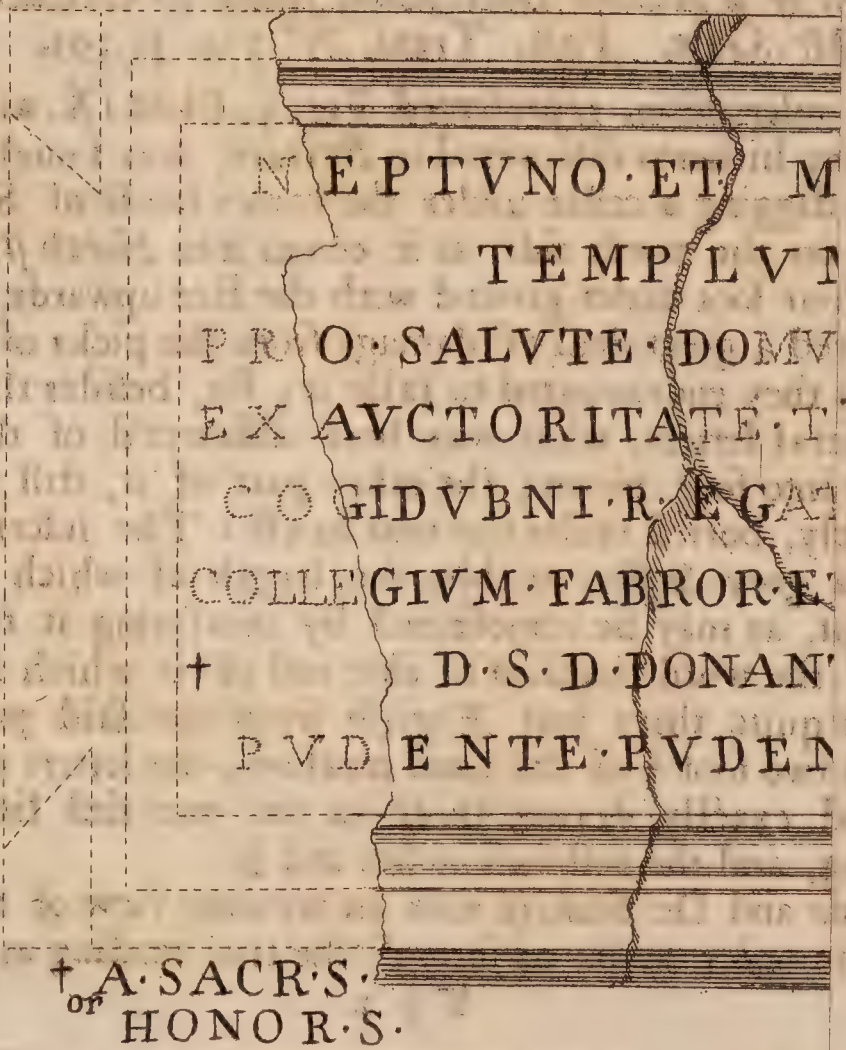
vagina,

Fig. I.

Fig. X.



Fig



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vagina, *uterus*, *rectum*, &c. wherein it had involved itſelf, as may be ſeen more particularly in the Fig. annexed.

A Fig. 1. Plate IX. repreſents the *clitoris*; B B the *nymphæ*; C the *rima* or entery into the *vagina*; D the bladder; E E the *ureters* cut off; F the *uterus* ſound and entire; G the left *ovarium*; H H the *tubæ*; I part of the *colon* cut off; K part of the *peritonæum*; L part of the *ileum* cut off; M part of the *cæcum*; O part of the *cranium* that plainly appears; P the ulcer, thro' which the *fæces* paſſed, with ſome ſmall bones; Q Q Q Q the bulky maſs, wherein the ſkeleton is contained between the *uterus*, part of the *vagina* and *rectum*; R R part of the *cutis*.

She was full nine months gone in *Auguſt* 1717, and ſhe died *March* 23, 1723, on which day the Dr. took it out.

Fig. 2. repreſents a lateral view; A the orifice of the *vagina*; B the *anus*; C the *rectum*; D D D the maſs wherein the ſkeleton is contained; E the ribs of the *fœtus* plainly appearing; F the bladder laid aſide; G the *ureter* cut off; H the *uterus*; I the *ovarium*; K the *tuba*; L part of the *colon*; M the ulcer, thro' which the excrements paſſed, about one inch below the navel; N bones in the orifice of the ulcer.

An Account of a Roman Inſcription, *found at* Chicheſter; *by Mr.* Gale. Phil. Tranſ. N° 379. p. 391.

THE inſcription, repreſented Fig. 3. Plate IX. as curious as any hitherto diſcovered in *Britain*, was found at *Chicheſter*, in digging a cellar under the corner houſe of St. *Martin's-lane*, on the north ſide as it comes into *North ſtreet*: It lay about four foot under ground with the face upwards, whereby it received a great deal of damage from the picks of the labourers, as they endeavoured to raiſe it; for, beſides the defacing of ſeveral letters, what was here diſinterr'd of the ſtone was broke into four pieces; the other part of it, ſtill wanting, is, probably, buried under the next houſe: The inſcription is cut upon a grey *Suſſex* marble, the length of which was ſix *Roman* foot, as may be conjectured by meaſuring it from the middle of the word *templum* to that end of it which is entire, and is not quite three foot *Engliſh* from the ſaid point; its breadth is two foot $\frac{3}{4}$ of the ſame meaſure; the letters are beautifully and exactly drawn, thoſe in the two firſt lines three inches long, and the reſt two inches and $\frac{1}{4}$.

Mr. *Gale* and Dr. *Stukely* took an accurate view of this marble, and in order to be as ſure of the true reading as poſſible,

wherever the letters were effaced, they impreſſed a paper with a wet ſponge into them, and by that means found thoſe in the fifth line to be, as expreſſed in the Fig. and not as in other copies, that have been handed about of this inſcription.

The only letter wanting in the firſt line is an N, before EPTVNO; and ſo there is no difficulty in reading that: As to the ſecond line, tho' it be more uſual in inſcriptions of this nature to expreſs the donation by the word SACRUM only, referring to the temple or altar dedicated; yet we have ſo many inſtances, in *Gruter's corpus Inſcriptionum*, of TEMPLUM and ARAM, alſo cut on the ſtones, that there is not the leaſt occaſion to ſay any thing farther upon that point.

The third line can be no otherways filled up, than Mr. *Gale* has done it by the pricked letters: He owns, however, that he had ſome ſcruple about the phraſe DOMUS DIVINA, the ſame thing as DOMUS AVGVSTA, the imperial family, which he cannot ſay occurs, with any certainty, as to the time it was uſed in, before the reign of *Antoninus Pius*, from whom down to *Conſtantine the Great*, it is very frequently met with in inſcriptions. This kept him ſome time in ſuſpenſe, whether this found at *Chicheſter* could be of ſo early a date, as the time of *Claudius*: But as we find ſeveral inſcriptions in *Gruter* with thoſe words in them, or J. H. D. D. *In honorem domûs divinæ*, which is much the ſame thing, without any mark of the time, when they were cut, they may have been before the reign of *Antoninus Pius*, and then only came into more general uſe; and as the time that *Cogidunus* lived in, will not ſuffer this to be of a later ſtanding, Mr. *Gale* thinks it may be offered as an authority for the uſe of this piece of flattery to the emperors, long before that excellent prince came to the purple.

The third line as Mr. *Gale* takes it, was EX AUCTORITATE. TIB. CLAUD. and the fourth COGIDUBNI R. LEG. &c. that is, *ex authoritate Tiberii Claudii Cogidubni Regis, Legati Auguſti in Britanniâ*, for the following reaſons; we are informed by *Tacitus* in *Vitâ Agricolæ* cap. 14. that after *Britain* had been reduced to a *Roman* province by the ſucceſsful arms of *Aulus Plautius* and *Oſtorius Scapula*, under the emperor *Claudius*; *Quædam civitates Cogiduno Regi erant donatæ; is ad noſtram uſque Memoriam fidiſſimus remanſit, vetere ac jam pridem receptâ Populi Romani conſuetudine, ut haberet inſtrumenta ſervitutis & Reges*: This *Cogidunus* ſeems to be the ſame perſon with *Cogi-*

dubnus

Iubnus in the inſcription, the letter B in the third ſyllable making little or no difference in the word; eſpecially if pronounced ſoft, as it ought to be, like an V conſonant.

It is ſo well known to have been the cuſtom of the *Roman Liberti* and *Clientes* to take the names of their patrons and benefactors, that it would be needleſs to prove the conſtant uſage of that practice: Now, as this *Cogidubnus*, who, in all probability, was a petty prince of that part of the *Dobuni*, that ſubmitted to *Claudius*, and one that continued many years faithful to him and the *Romans*, vide *Tacitus ut ſupra*, had the government of ſome part of the iſland given him by that emperor, nothing could be more grateful in regard to *Claudius*, nor more honourable to himſelf, after he was *romanis'd*, than to aſſume the names of a benefactor, to whom he was indebted for his kingdom; and ſo called himſelf TIBERIUS CLAUDIUS COGIDUBNUS.

Mr. *Gale* ſuppoſes him to have been a *Regulus* of the *Dobuni*; becauſe we are told by *Dion Caſſius lib.* 60, that *Aulus Plautius* having put to flight *Cataratacus* and *Togodumnus*, ſons of *Cunobelin*; part of the *Boduni* (the ſame people as the *Dobuni*) who were ſubject to the *Catuellani*, ſubmitted to the *Romans*; and the name *Cogidubnus* or *Cogiduvnus* Cot o Dubn or Duvn, vide *Baxteri Gloſſar. in verbis* COGIDUMNUS and DOBUNI, ſignifying expreſsly in the *Britiſh* language *Princeps Dobunorum*, ſeems to put the matter out of all doubt.

How far his territories extended, it is impoſſible to determine: Biſhop *Stillingfleet Orig. Britan. p.* 63. ſuppoſes them to have lain in *Surrey* and *Suſſex*; *Suſſex* was certainly part of them; ſince the temple, mentioned in this inſcription, was erected in it by his authority; and it is not unlikely, that beſides the *Regni*, who were the people of thoſe two counties, he might have aſſigned to him that part of the *Dobuni* that had ſubmitted to the *Romans*, which ſeems to have been his own principality, together with the *Ancalites*, *Bibroci* and *Segontiaci*, whoſe countries lay between the *Dobuni* and *Regni*; the words *Civitates quædam* in *Tacitus*, not importing ſome few towns, but ſeveral people; the word *civitas* always ſignifying a people in that hiſtorian.

Before Mr. *Gale* proceeds any farther, he obſerves, that *Togodumnus* and *Cogidumnus* (tho' their names be ſo much alike) were two diſtinct perſons; the firſt, ſon of *Cunobelin*, king of the *Trinobantes*, vanquiſhed and kill'd in battle by *Aulus Plautius*;

Plautius; the ſecond a prince that ſubmitted to *Oſtorius Scapula*, and continued in his fidelity to the *Romans* in *noſtram uſque memoriam*, ſays *Tacitus*, and who was born at the latter end of *Claudius*'s reign: So that *Togodumnus* was, probably, dead, before *Cogidumnus* had his government conferred upon him.

Mr. *Gale* calls it his government: For, tho' by the letter .R. in the inſcription, with a point both before and after it (whereby it plainly denotes an entire word of itſelf) it may ſeem that it was intended for COGIDUBNI REGIS; and he believes was ſo with reſpect to his former dignity; yet it is evident, that he had condeſcended to take the title of LEGATUS AUGUSTI IN BRITANNIA from *Claudius*; and that too muſt have been only over thoſe people, the government of whom was given him: *Aulus Plautius*, *Oſtorius*, *Scapula*, *Didius Gallus*, *Avitus Veranius* and *Suetonius Paulinus*, having about this time the ſupreme command, ſucceſſively in this iſland; the ſecond and laſt of which are expreſsly called *Legati* by *Tacitus*, *lib.* 12. *Ann. cap.* 32 and *Vit. Agricol. cap.* 15. The *Legati Cæſaris* or *Auguſti* were thoſe, *qui Cæſaribus ſubditas regebant Provincias.*

The ſixth line has loſt at the beginning the letters COLLE; but ſo much remains of the word as makes it, undoubtedly, to have been, when entire, COLLEGIUM, and the following letters are an abbreviation of FABRORUM.

Theſe colleges of artificers were very ancient at *Rome*, even as ancient, as their ſecond king *Numa Pompilius*, if we may believe *Plutarch* in *Vit. Numæ*, who tells us, that the people were divided by him into what we at this day call *Companies of tradeſmen*, and he mentions the Τέκτονες or *Fabri* among them; tho' *Florus* lib. 1. cap. 6. ſays, that *Populus* Romanus *a Servio Tullio relatus fuit in cenſum, digeſtus in claſſes, curiis atque Collegiis diſtributus.* But as the power of the *Romans* extended itſelf, it carried the arts of that great people along with it, and improved the nations it ſubdued, by civilizing and teaching them the uſe of whatever was neceſſary or advantagious among their conquerors; from which moſt wiſe and generous diſpoſition, among other beneficial inſtitutions, we find theſe *Collegia* to have been eſtabliſhed in every part of the empire, from the frequent mention of them in the inſcriptions, collected by *Grater*, *Spon* and other antiquaries.

Several ſorts of workmen were included under the name of *Fabri*, particularly all thoſe concerned in any kind of building;

ing; whence we meet with the *Fabri Ferrarii*, *Lignarii*, *Tignarii*, *Materiarii*, *Navales* and others: The *Navales* may have been the authors of dedicating this temple to *Neptune*, having ſo near a relation to the ſea, from which the city of *Chicheſter* is at ſo ſmall a diſtance, that, perhaps, that arm of it, which ſtill comes up within two miles of its walls, might formerly have waſhed them: The reſt of the fraternity might very well pay the ſame devotion to *Minerva*, the Goddeſs of all arts and ſciences, and patroneſs of the *Dædalian* profeſſion.

As no leſs than five letters are wanting at the beginning of the ſixth line; there cannot be fewer loſt at the beginning of the ſeventh, where the ſtone is more broke away than above; ſo that, probably, there were ſix when it was entire. What is left of them is only the top of an S: Mr. *Gale* does not, therefore, take upon him to affirm any thing, as to the reading of them, being ſo entirely defaced: Perhaps, it was, A SACR. S. *i. e.* A *ſacris ſunt*; or perhaps HONOR. S. i. e. *Honorati ſunt*: As to the former, we find theſe *Collegia* had their *Sacerdotes*; therefore, *qui a ſacris ſunt*, which is found in inſcriptions (vide *Gruter Corp.* 29.8—121.1—632.1.) would be no improper term to expreſs them; or it might have been SACER. S. i. e. *Sacerdotes ſunt*, ſince we find ſuch mentioned in the following inſcriptions in *Spon's Miſcell. Erudit. Antiquit.* p. 58.

MAVORTI SACRUM
HOC SIGNUM
RESTIT. . . . .
COLL. FABR. ARI
CINORUM ANTIQUISS.
VETUSTATE
DILAPSUM ET
REFECER. CUR. L. LUCILIUS
LATINUS PROC. R. P. ARIC
ET T. SEXTIUS MAGGIUS
SACER. COLL. EJUSD.

i. e. *Mavorti ſacrum hoc ſignum reſtituit Collegium* Fabrorum Aricinorum *antiquiſſimum, vetuſtate dilapſum, & refecerunt. Curabant Lucius Lucilius Latinus, Procurator Reipublicæ Aricinorum, & Titus Sextius Maggius* Sacerdos *Collegii ejuſdem.*

Ibid.

Ibid. p. 64. in the following inſcription

L. TERTENI AMANTI
SACER. COLL. LOTORUM
II VIR. C. SARTIUS C. F.
ITERINUS ET L. ALLIUS
PETELINUS D. D.

i. e. *Lucius Tertenius Amantius* Sacerdos *Collegii Lotorum, Duumviri Caius Sartius, Caii filius, Iterinus, & Lucius Allius Petelinus dedicaverunt.*

As to the latter, thoſe members of the college, that had paſſed thro' its chief offices, as that of *Præfectus* or *Magiſter quinquennalis*, had the title of HONORATI conferred upon them: Several of theſe HONORATI are mentioned in *Gruter*; particularly a long catalogue of them *in Collegio Fabrorum Tignariorum*, p. 268. 1. and in *Reineſius's ſyntagma* p. 605. there is the following inſcription.

EPAGATHO TURANNO
HONORATO COLLEGI
FABRUM TIGNARIORUM
ROMANENSIUM, &c.

So that the vacuity in our inſcription might very well be filled up with one or other of theſe words, and the three next lines that follow them D. S. D. i. e. *de ſuo dedicaverunt*, will agree with either of them and with what preceeds them.

The laſt line has been PUDENTE PUDENTINI FILio; but there muſt have been a letter or two of the *prænomen* at the beginning of it; unleſs it were ſhorter than the reſt at that, as well as at the latter end of it; and from what Mr. *Gale* has ſaid, the whole may be read, as follows.

Neptuno & Minervæ Templum pro ſalute domus divinæ, ex authoritate Tiberii Claudii Cogidubni Regis, Legati Auguſti in Britanniâ, Collegium Fabrorum, & qui in eo a ſacris (or *honorati*) *ſunt de ſuo dedicaverunt, donante aream Pudente Pudentini filio.*

Chicheſter, by this inſcription found at it, muſt have been a town of note, very ſoon after the *Romans* had ſettled here, and in proceſs of time it ſeems to have been much frequented, by the *Roman* roads, ſtill viſible, that terminate here from *Portſmouth*,

mouth, *Midhurſt* and *Arundel*; tho', what is very ſtrange, we have no *Roman* name now known for it. Mr. *Gale* once thought it might have put in its claim for *Anderida*, which our Antiquaries have not yet agreed to fix any where, being ſituated very near, both to the *Sylva Anderida* and the ſouthern coaſt of the iſland, the two properties of that city, vide *Camd. Brit.* and *Somner's Roman Ports and Forts*: But *Henry* of *Huntingdon*, who liv'd in the time of *Henry* II. telling us, p. 312. that the *Saxons* ſo deſtroy'd *Andredeceſter*, that *nunquam poſtea reædificata fuit, & locus tantum quaſi nobiliſſimæ urbis tranſeuntibus oſtenditur deſolatus*, vide *Phil. Tranſ.* N° 356, it could not be *Chicheſter*: For, that was not only rebuilt before his time, but a place of ſuch note, that when the Biſhops ſoon after the conqueſt A. D. 1076, remov'd their churches from ſmall decay'd towns, where ſeveral of them were then ſeated, *in urbes celebriores*; *Stigand*, then Biſhop of *Selſey*, ſettled his Epiſcopal chair at that place.

Mr. *Gale* concludes with obſerving, that when this inſcription was dug up, there were alſo two walls of ſtone, diſcover'd cloſe by it, each three foot thick; one running north, the other eaſt, and joining in an angle, as *North-ſtreet* and St. *Martins-lane* now turn, which, in all probability, were part of the foundations of the temple, mentioned on the marble.

Of the Structure of the Diaphragm; *by M.* Leewenhoeck. Phil. Tranſ. N° 379. p. 400. *Tranſlated from the* Latin.

IN *Phil. Tranſ.* N° 377. M. *Leewenhoeck* gave his opinion about a diſorder he was ſies'd with, which he ſuppos'd had been owing to the bad diſpoſition of his diaphragm.

In order to remove all doubts about this matter, he procured the diaphragm of a ſheep a year old, part of which he cut into ſmall pieces, and viewing them very carefully with his microſcope, he found, that the diaphragm conſiſts partly of very fine fibrils, which appear to the naked eye to be about a hair's breadth diſtant from each other.

Upon placing different pieces of the diaphragm before different microſcopes, and carefully viewing them, he poſitively concluded, that theſe fibrils aroſe from fleſhy parts, interwoven with the diaphragm about the ribs; and ſuppos'd, that the ſaid fibrils ſerv'd the diaphragm inſtead of tendons:

Theſe tendons, as M. *Leewenhoeck* calls them, are not equally near each other throughout the whole diaphragm; for ſome are a little thicker than others, tho' near each other.

Upon duly conſidering the laſt mentioned fibrils and the membrane extended between them, he found, that the greateſt part of the diaphragm conſiſted of theſe fibrils and the ſaid membrane; beſides a great number of blood-veſſels, and a large quantity of fat contain'd therein.

M. *Leewenhoeck* was very much ſurpris'd at the vaſt number of little *rugæ* or folds in the membrane between the pretended tendons; theſe *rugæ* he ſuppoſed deſign'd by nature for contracting the diaphragm, when extended in inſpiration, and by that means contributing to expel the air, convey'd into the lungs.

To ſhew the contexture of the diaphragm, he caus'd delineate a ſmall portion of the diaphragm of the ſaid ſheep, which is repreſented by A B C D E F G H, Fig. 4. Plate IX; where B G, C F and D E are three tendons, as they appear'd thro' the microſcope: Between theſe tendons may be ſeen the intermediate membrane, of which the firſt tendons, (as has been already obſerv'd) the greateſt part of the diaphragm conſiſts.

In order to form a judgment of the true bigneſs of Fig. 4. the real ſize of the piece delineated is repreſented by the ſpace between Y Z in Fig. 7.

Moreover, he obſerv'd, that the ſaid membrane was perforated with a vaſt many, and thoſe very ſmall holes, which opened from the *abdomen* into the breaſt, and from the breaſt again into the *abdomen*. Being hitherto of opinion, that it was impoſſible for any liquor to be convey'd from the breaſt into the *abdomen*, or from the *abdomen* into the breaſt, he now thought it better, to ſuſpend his judgment about that matter: Yet he reaſoned thus, there are, perhaps, inconceivably ſmall veſſels in our lungs; for, if we conſider, how great a quantity of moiſture is expir'd out of our lungs into the open air, in compariſon of the ſmall quantity inſpir'd into them, we will eaſily incline to that opinion.

M. *Leewenhoeck* ſeveral times obſerv'd, that ſome veſſels creep tranſverſely over thoſe fibrils, he calls tendons.

In another piece of the ſame diaphragm, that ſtood before his microſcope, he could plainly obſerve the ſtructure and origin of the little membranes, which, as has been ſaid,

lay

lay between the fibrils or pretended tendons, and were furniſh'd with a vaſt many folds, as has been likewiſe obſerv'd; all which are repreſented at LK Fig. 5. but the fibrils or ſmall tendons at LMNOPQ; tho' theſe fibrils were larger here, than ever he had obſerv'd before.

Moreover, he tore that membrane aſunder (of which the greateſt part of the diaphragm conſiſts) notwithſtanding it was ſo thin; a ſmall portion of which he caus'd to be delineated, as it appear'd thro' the microſcope, and repreſented Fig. 6. This he did in order to ſhew (as much as poſſible) the torn tendons and membranes; a ſmall portion of an extended tendon is repreſented by RSTV, where are ſhewn the very minute fibrils, of which the ſaid portion of the tendon conſiſts: In the ſame figure VT and WX repreſent a portion of the tendon at reſt.

It is ſcarce poſſible to explain, or ſufficiently comprehend, what an incredible number of fibrils, veſſels and particles are here join'd together and contribute to form that very fine membrane, the diaphragm. M. *Leewenhoeck* ſuppoſes, that in ſtrong inſpiration, all theſe *rugæ* or corrugated *Sinus*'s in the diaphragm become flat and plain; and that in evaporation, the diaphragm is again corrugated, as was obſerv'd above.

While, therefore, we ſee ſo great a quantity of fat generated, and collected in the diaphragm; and moreover conſider, that the ſubſtance of the diaphragm may on both ſides become ſo thick with fat, as to ſwell out; we may eaſily conceive, how the expanſion and contraction of the diaphragm may by that means be hinder'd and retarded; and conſequently, a difficulty of breathing ariſe: And this is the reaſon that fat people are ſhort winded. Yet that the diaphragm be lin'd with ſome fat is neceſſary, as it expands 90 times in an hour, and as often contracts; and theſe ſeveral motions are much aſſiſted by the fat.

Tho' M. *Leewenhoeck* ſeveral times endeavoured to diſcover, how the fleſhy parts of the diaphragm, which (as has been ſaid) lie next the ribs, became a membrane, or how the membrane of the diaphragm aroſe from them; after ſeveral trials, he could not ſatisfy himſelf.

Now, he began to ſuſpect, whether theſe tendons were not furniſh'd with veſſels; and tho' he could hardly promiſe ſo much to himſelf from his microſcope; yet the third day after, he repeated his obſervations, and cut the membranes

tranſverſely, in order to diſcover the blood-veſſels in the membrane of the diaphragm, which, as has been ſaid, was full of *rugæ*: At length he obſerv'd blood-veſſels, extended along the tendons, nay, 8 or 10 cloſe together: Afterwards he turn'd his eyes to view the membrane, that covers the tendons, and here he obſerv'd a vaſt number of very ſmall veſſels, many of which lay included in *areolæ*, as it were; and M. *Leewenhoeck* judged them to be the veſſels of the tendons cut tranſverſely. In order to have an eye-witneſs of all this, he gave the microſcope (before which the ſaid portion of the diaphragm ſtood) to his ſurgeon; who affirm'd, that M. *Leewenhoeck*'s deſcription exactly agreed with what he ſaw.

M. *Leewenhoeck*, not ſatisfied with this obſervation, procur'd the diaphragm of an ox, from which cutting off about the breadth of a palm, at the fleſhy parts, next the ribs, he found it about four times thicker than that of a ſheep, and that this thickneſs is moſtly owing to the fat.

Afterwards he cut off a thin ſlice, and as often as he cut off one ſomewhat thicker, he cut thro' fat, extending towards the middle of the diaphragm, where the diaphragm is pretty thick, and has ſome fat. The diaphragm on both ſides, that is, in its upper and lower parts, conſiſts of four diſtinct membranes and their tendons, which laſt lie inclos'd, as it were, within the membranes.

M. *Leewenhoeck* was ſurpris'd at the vaſt number of theſe tendons; nor did it ſeem credible, that there ſhould be ſuch variety of parts in a body, as the diaphragm, whoſe ſtructure and functions are ſo little known.

To ſhew the ſtructure of the diaphragm of an ox, he caus'd part thereof to be delineated, as repreſented by ABCD Fig. 8. where are ſhewn firſt four parts of diſtinct tendons, which lie cloſe to each other in great numbers, and are likewiſe repreſented in the figure of the ſheep's diaphragm. But ſuch minute tendons (as he calls them) are ſpread throughout the whole ſubſtance of the diaphragm of an ox, which, together with the membranes, muſt neceſſarily be alternately expanded and contracted in a well diſpos'd body; all which is pretty accurately delineated in Fig. 8.

M. *Leewenhoeck*, by repeated obſervations, likewiſe found to his ſatisfaction, in the diaghragm of a ſheep, thoſe very fine fibrils, as alſo the ſmall *rugæ* or folds of the membranes, which he did not only himſelf obſerve with pleaſure, but

ſhew'd

ſhew'd them to others. Moreover, cutting theſe tendons, and very thin particles tranſverſely, in order to find, whether they were perforated and furniſh'd with cavities, he obſerv'd ſuch a vaſt number of very ſmall veſſels, as is ſcarce credible to any, who had not ſeen them, running acroſs the diaphragm; and as far as he could judge, deſign'd by nature to convey the fat thro' all its parts, and ſupply the diaphragm with conſtant nouriſhment.

M. *Leewenhoeck* obſerv'd above, that the fat lay inclos'd in the diaphragm, and that four diſtinct membranes cover it on both ſides; and theſe are ſo cloſely join'd together, that they ſeem'd to be but one ſingle membrane.

M. *Leewenhoeck* placed before another microſcope (that magnified ſomething leſs than the former) a piece of the diaphragm of a mean thickneſs; in order to ſhew, how the membranes ſurrounded on both ſides the fatty particles, or the fat of the diaphragm, ſo that it ſeems to be inclos'd within theſe membranes: This is repreſented by EFGHI KLM Fig. 9. where EFGH and IKLM ſhew the two membranes that conſiſt of four parts each; GH and IK the place where the fat, ſo often mentioned, is inclos'd.

Afterwards M. *Leewenhoeck* endeavouring to cut into round pieces a very ſmall portion of the diaphragm, it ſeparated into two parts or *lamellæ*; whence he ſuſpected, that the diaphragm was ſo form'd by nature, for the more eaſily diſtribution of the fat, thro' the ſubſtance of the diaphragm.

In Fig. 9. where the membranes are repreſented cut tranſverſely, there appear ſome ſmall corrugations, which he moſtly obſerv'd in them. Afterwards he found that the little membranes, where the fat is collected, were faſtened to very minute veſſels, which by the exſiccation of the contiguous parts were broken aſunder. M. *Leewenhoeck* takes the connection of theſe little membranes and veſſels to be neceſſary on this account, *viz.* that the diaphragm, as often as it is extended, may not become quite flat, but almoſt expand into a globoſe cavity.

A great many people, may poſſibly think, that the diaphragm is a very thick membrane; whereas it is a very thin one; and to ſhew this, he caus'd delineate a little bit of the diaphragm of an ox, as it appears to the naked eye: It is true, the diaphragm is very ſtrong, conſidering its thickneſs; which he takes to be owing to the great number of fine tendons.

NOPQ

NOPQ Fig. 10. repreſents a little bit of the diaphragm of an ox; where the fibril NO repreſents one ſide or half the thickneſs of the diaphragm; PQ the other half: The reſt of the diaphragm, to wit, what lies between OP and NQ, is ſurrounded with fat or fatty particles; only that there lies ſome fleſh between NQ, belonging to that next the ribs.

The oftener therefore, M. *Leewenhoeck* reflected upon the abovementioned diſorder he was ſeiſed with, and which he attributed in a great meaſure to the diaphragm, he was perſuaded phyſicians were miſtaken, when they call the palpitation, we ſometimes perceive in the region of the breaſt, a palpitation of the heart. M. *Leewenhoeck* takes ſuch palpitations to ariſe from the bad diſpoſition of the diaphragm; whether it be owing to a want of nouriſhment, or to an obſtruction in ſome of theſe veſſels, which are very numerous throughout the diaphragm: For, ſuch an obſtruction may eaſily cauſe convulſive motions in the abovementioned tendons; and M. *Leewenhoeck* takes this to have been the very cauſe of his diſorder.

A preternatural Structure of the Parts of Generation *in a* Woman; *by Dr.* Huxham. Phil. Tranſ. N° 379. p. 408. *Tranſlated from the* Latin.

ONE *A. B.* of the pariſh of *Lanteglaſs*, in the county of *Cornwall*, near *Fowye*, was married at 25 years of age; after which ſhe conceived, and being conſcious of the bad conformation of the genital parts, ſhe had recourſe to Mr. *Bonnett* of *Fowye*, a ſurgeon and man-midwife, who made the following obſervations.

In the place of the navel, in the middle of the *abdomen*, but a little lower, there was a prominent ſpongy maſs, reſembling a lump of fleſh, lying tranſverſely upon the *abdomen*, almoſt as big as a hen's egg and three fingers in length; with two ſmall urinary paſſages from which the urine continually ouz'd, ſo that ſhe could neither retain nor ſquirt it out; hence we may conclude, that the bladder (if there were any) wanted a *ſphincter*. This ſpongy ſoft maſs, being corroded with the acrimony of the urine, could hardly bear the gentleſt touch; ſo that ſhe was obliged to walk double, to avoid rubbing againſt it with her cloaths, and to wrap it up in ſoft linnen: Dr. *Huxham* takes this maſs to be the navel-ſtring, that was unſkillfully cut and as unſkillfully cured: For, there did not appear the leaſt ſign of a navel, any where elſe but here; and either the urine flow'd thro' the *urachus* which was perforated, and, probably divided

into

into two urinary paſſages, or at leaſt thro' two peculiar canals; however he rather inclines to the former opinion; becauſe we have accounts of the voiding of urine by the navel, and that in adults, vide *Hiſt. de l'Academie Roy. des Scien. Ann.* 1701.

A little way, below this maſs of fleſh was the orifice of the *vagina*; from which flowed the *menſes*; and by which ſhe was impregnated; tho' with little pleaſure in the time of coition, the extremity of the *glans* (much leſs the *penis* itſelf) being ſcarce able to enter this orifice: And the ſurgeon could hardly introduce his finger into it, in order to diſcover the neck of the *uterus*, which he could by no means feel; but he plainly felt, a thick membrane, that ſeparated this orifice from one lower, to be deſcribed anon.

In the ſame place almoſt but ſomewhat higher, where in other women is the *foſſa magna*, was that other oblong orifice, ſcarcely admitting the tip of the little finger, pervious to the *rectum*, as was obſerved after the delivery, and this might, probably, be owing to the inciſion; no *ſphincter* was obſerved, but below that, the *rectum* terminated, as uſual, with its *ſphincter*.

This oblong orifice was almoſt two fingers diſtant from the orifice of the *vagina*, when the *abdomen* was moſt ſwelled; and between theſe intervened the abovementioned membrane internally, but the coalition, as it were, of the *labia* of this oblong fiſſure from the ſuperior part of the orifice externally.

There was neither *clitoris* nor *oſſa pubis*, unleſs, the ſhort *apophyſes*, jutting out from the lower part of each *os ilium*, might be called the rudiments of them. This was the caſe before delivery.

July 18. 1722, Mr. *Bonnett* was called at 11 o'clock at night, to aſſiſt in the delivery. Upon thoroughly conſidering every particular circumſtance, he found the *fœtus* fallen below the orifice of the *vagina*, and in vain he attempted to thruſt it up, by putting the woman in a proper poſture, by reaſon of the ſtrong motion of the *fœtus* and violent throws of the mother, who was now ſeiſed with convulſions, *ſyncope*, *&c.* The orifice of the *vagina* was ſcarce, if at all, dilated; ſo that every one expected ſhe would immediately expire.

In this melancholly ſituation, Mr. *Bonnett* was of opinion, that a doubtful remedy was to be preferred to none at all, ſince otherwiſe death was unavoidable; introducing his ſcalpel into the lower oblong orifice, he at once divided the coalition of the *labia*, and the intermediate membrane; ſo that the orifice of the *vagina* and the lower oblong orifice now became one: Upon this,

this, he eaſily introduced his fingers, felt the internal orifice of the *uterus*, and dilated it a little, ſo that he could now feel the head of the *fœtus:* In ſhort, putting his finger into its mouth he at length extracted a live female child to the great ſurpriſe of the by-ſtanders.

From the time of her delivery ſhe was troubled with a *prolapſus uteri*; if not reduced for 8 or 10 hours and afterwards forcibly thruſt up, from the abovementioned urinary paſſages two ſtreams would ſquirt out to the diſtance of at leaſt a foot; a manifeſt proof that there was ſome *cyſtis* for receiving the urine; otherwiſe the Dr. ſuſpects they might be the orifices of the *ureters*, that terminated there.

Should any one aſk, how this woman was impregnated? To this the Dr. anſwers, that the immiſſion of the *penis* is not abſolutely neceſſary in procreation, tho' the injection of the *ſemen* into the *vagina* be abſolutely ſo; vide *Hiſt. de l'Acad. Royale des Sciences An.* 1712. as alſo *Mauricedu.*

A A Fig. 11. Plate IX. ſhews the ſpongy maſs of fleſh (which the Dr. takes to have been part of the navel-ſtring) almoſt of the bigneſs of a hen's egg and three fingers in length; in it were two urinary paſſages *b b*, from whence the urine continually ouz'd, without being voided by any other orifice; *c* the orifice of the *vagina*, very much reſembling the *anus* of a cock, and ſurrounded with a ſtrong *ſepium*, as it were; by this the *menſes* flowed and by it the woman was impregnated: This orifice, in the moſt violent throws, was ſcarcely at all dilated, tho' the *anus* was very much, from the violent contractions of the muſcles of the *abdomen*, *&c. d* the place of the oblong orifice, ſcarce admitting the tip of the little finger; and into this (at the time of delivery) Mr. *Bonnett*, introducing his ſcalpel, cut it up as high as the ſuperior orifice; ſo that now there was a prodigious large aperture here: The upper tranſverſe orifice on the approach of the birth, was almoſt two fingers diſtant from the lower oblong orifice, Thro' this large aperture ſhe often ſuffered a *prolapſus uteri:* From the time of the delivery, the *fœces* were partly voided by that part where the lower orifice was formerly; but this was not obſerved before the delivery, ſo that, probably, it was owing to the operation. This oblong orifice is repreſented in the Fig. ſomewhat higher than the life; *e* repreſents the *anus* a little more forwards than uſual; by which the greateſt part of the *fœces* was diſcharged: The *pudenda* was not thick of hair.

An

An Account of the same Woman; *by Dr.* Oliver. Phil. Tranſ. N° 379. p. 413. *Tranſlated from the* Latin.

THIS woman was formed in the following manner; a kind of fleſhy, ſpongy ſubſtance of a florid red colour, that could not bear the leaſt touch, without the moſt exquiſite pain, poſſeſſed the place of the navel, and ſprung, as it were, out of the cavity, which is generally in that part. From this excreſcence (which did not ſenſibly increaſe or diminiſh for ſeveral years) the urine continually ouz'd without having any other paſſage; nor could it by this with the ſtrongeſt effort be ſquirted out: A little below, at almoſt the diſtance of a ſmall palm, there was a ſort of paſſage, an inch in length and a finger in breadth, large enough to admit the forefinger and yet keep it tight. Her *menſes* flow'd by this paſſage; whence it plainly appeared, this was the orifice to the *vagina* and *uterus*: At almoſt the ſame diſtance, as the orifice of the *vagina* from the excreſcence, was another lower orifice, leſs than the upper, and ſcarce admitting the little finger: It was evident, that this orifice communicated with the *rectum*, from the thinneſt parts of the *fæces* being frequently voided thereby: The true *anus*, by which the harder *fæces* were diſcharged, was five or ſix inches diſtant from this falſe *anus*, and almoſt in the place where the *pudendum* naturally is. There were ſome few hairs, deſcribing two lines, which began from the middle ſpace between the excreſcence and the commiſſures of the thighs with the *abdomen*, and approaching each other formed an angle, in whoſe vertex the true *anus* was ſituated: She was afterwards married and proved with child; the orifice of the *vagina* was not at all dilated by coition; ſo that there aroſe ſeveral diſputes among the women about the manner of the impregnation. That the emiſſion of the *penis* is not abſolutely neceſſary in impregnation, we have ſeveral inſtances: However ſhe applied to a ſurgeon at *Foy*, for his advice, who, upon hearing the caſe and examining into the matter, promiſed his aſſiſtance, if neceſſary: Her time now drawing near, ſhe began to feel her pains and the other ſymptoms. The ſurgeon being called, affirmed, that the *fœtus* made towards the region of the *pubis*; but ſhe herſelf, towards the orifice of the *vagina*. In ſhort; the ſurgeon inſiſted, that an inciſion muſt be made to open a paſſage for the child; this the by-ſtanders oppoſed, but her pains increaſing, ſhe was ſeiſed with convulſions and her life was deſpaired of. At length when all concluded ſhe was now near her end, they

delivered her up almoſt expiring into the hands of the ſurgeon, who immediately reſolved to cut the *iſthmus*, between the orifice of the *vagina* and falſe *anus*, and which may properly enough be called the *perinæum*: After cutting the external integuments a ſtrong thick membrane, placed tranſverſely, preſented itſelf to view, which ſeparated the *vagina* from the lower *canalicule*: Upon cutting this aſunder, there was made a pretty large paſſage to the *uterus*, and introducing his hand he dilated the *o. uteri*, laid hold of the child by the head, extracted it ſafe and recovered the mother to life. The Dr. himſelf went to *Foy* to know the truth of the matter, and the ſurgeon informed him both of the original ſtructure of the parts and of the operation exactly in the manner above narrated, all which the woman herſelf confirm'd: And upon examining her, he found, that the excreſcence at the navel exactly agreed with the above deſcription; that upon the leaſt touch, ſhe would ſcream out as if run thro' with a ſword; and being aſked how ſhe could bear the weight of her husband in coition, ſhe replied the love ſhe bore her husband made every thing eaſy to her.

But to return; this excreſcence ſeemed to conſiſt of ſeveral lobules, envelopp'd in their proper membranes, and diſtinct from each other: In the interſtices of theſe lobules, the ſmall pores thro' which the urine ouzed, opened at the ſuperficies. A little below appeared the large orifice made by the ſurgeon for facilitating the birth; on which account ſhe was ever after troubled with a *prolapſus uteri*, the *uterus* hanging down ſix inches and as big as a gooſe egg; and indeed, it would have been ſurpriſing had it been otherwiſe, the aperture at the orifice of the *vagina* being ſo very large, and the bladder preſſing more than uſual upon the *uterus*: It is probable, that the urine aſcended thro' the *urachus* towards that excreſcence, and originally flowed thro' it; becauſe nature had provided no exit thro' the urinary paſſage: Yet it is impoſſible, that the urine ſhould thus aſcend to the navel unleſs the bladder be very full; and in that caſe it muſt needs continually preſs upon the *uterus* and force it out: The better to obſerve the conformation of the parts ſhe was put in a reclining poſture on the bed, and attempting to thruſt up the *uterus*, it was no ſooner done than the urine ſquirted out in ſeveral ſtreams as thick as a hair thro' the pores of the excreſcence to the diſtance of ſeveral feet, like ſo many jets: After the bladder was emptied, the *vagina* was eaſily replaced; and after the *vagina* and *uterus* were both returned there appeared a large chaſm, three inches in diameter, whoſe

fimbriæ

Fig. I.

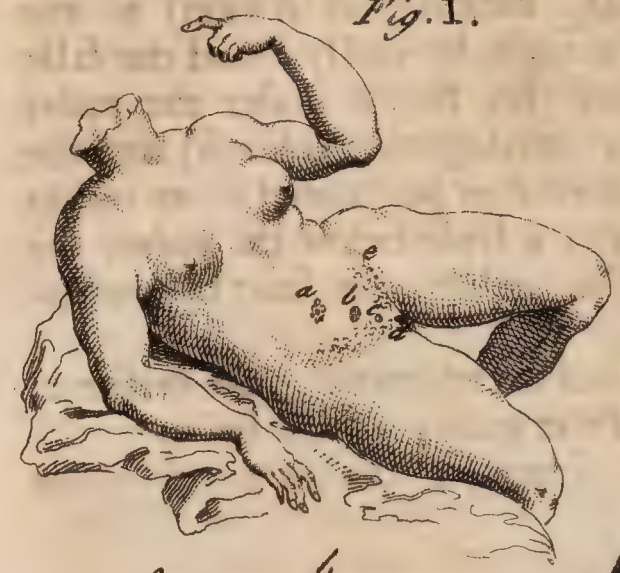


Fig. II.

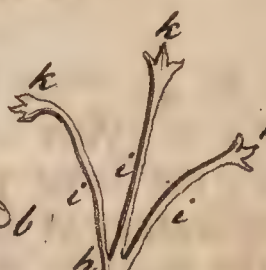


Fig. III.

Fig. VIII.

Fig. IV.

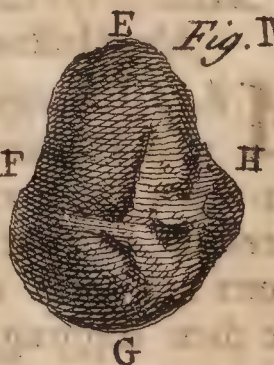


Fig. VI.

Fig. V.

Fig. VII.

fimbriæ rather than *labia*, had ſeveral unequal *ſarcoma*'s: In its upper internal margin the Dr. look'd (but in vain) for the urinary paſſage; in the lower margin he eaſily found the falſe *anus*, thro' which he introduced his finger under the *pubes* (which ſeemed to approach to a cartilaginous ſoftneſs) into the *rectum*; whence he ſuppoſes the *rectum* perforated three inches above the *ſphincter* of the *anus*; a little below the *pubes*, the true *anus* was ſituated, in the place where the *ſinus* of the *pudenda* is in other women.

a Fig. 1. Plate X. the umbilical excreſcence; *b* the orifice of the *vagina*; *c* the falſe *anus*; *d* the *anus*; *e e* the hairs.

a Fig. 2. the umbilical excreſcence; *b* the *os uteri*, *c* the *anus*.

A ſurpriſing Flux of Blood *thro' the* Penis; *by Dr.* Howman. Phil. Tranſ. N° 379. p. 418. *Tranſlated from the* Latin.

A Citizen of *Norwich*, 40 years of age, in perfect health, was ſeiſed the 30th of *June* 1716 with a flux of pure blood thro' the *urethra*; the ſame phænomenon returned the 31. of *July*: By the application of ſome medicines and drawing blood in the arm, the flux was ſtopped, till the 8. of *September* and afterwards it entirely ceaſed: It is remarkable, that the abovementioned flux happened without any previous pain, or ſubſequent dejection of ſpirits.

An Account of the Pits *for* Fullers-earth *in* Bedfordſhire; *by Mr.* Holloway. Phil. Tranſ. N° 379. p. 419.

MR. *Holloway* went to the fullers-earth pits at *Wavendon* near *Woburn* in *Bedfordſhire*, where there are ſeveral of them open; and as the men were then at work only in one and underſtanding that the earth was diſpos'd in much the ſame manner in all, he went down in no more than that wherein they were then digging, where he found things diſpoſed in the following manner.

From the ſurface, for about ſix yards depth, there are ſeveral layers of ſand, all reddiſh, but ſome lighter coloured than others, under which there is a thin *ſtratum* of red ſand-ſtone, which the workmen break thro'; and then for the depth of about ſeven or eight yards more, you have ſand again, and after that you come to the fullers-earth; the upper layer of which, being about a foot deep, the diggers call the *cledge*, which is thrown by as uſeleſs, by reaſon of its too great admixture with

the neighbouring ſand, which covers and has inſinuated itſelf into it; after this they dig up earth for uſe, to the depth of about eight feet more; the matter of which is diſtinguiſhed into ſeveral layers; there being commonly about a foot and a half between one horizontal fiſſure and another: The upper half of theſe layers of fullers-earth, where the earth breaks of itſelf, is tinged red, as it ſeems by the running of water from the ſandy *ſtrata* above; and this part they call the *crop*; between which and the *cledge* abovementioned, there is a thin layer of matter, not an inch in depth; in taſte, colour and conſiſtence not unlike *terra Japonica*; the lower half of the layers of fuller's-earth, they call the *earth-wall*; this is not tinged with the abovementioned red colour, and ſeems to be the more pure and the fitter for *fulling*; and underneath all is a layer of white rough ſtone, of about two foot in thickneſs; which, if they dig thro', as they very ſeldom do, they find ſand again, and then there's an end of their works.

There is one thing obſervable in the ſite of this earth, *viz.* that it ſeems to have every where a pretty equal horizontal level; becauſe they ſay, that when the ſand-ridges at the ſurface are higher, the fullers-earth lies proportionably deeper.

In theſe works they ſeldom undermine the ground; but as they dig away the earth below, others are employed to dig and carry away the ſurface; otherwiſe, the matter above, being of ſo light and flitting a nature, would fall in and endanger the workmen: For, as was obſerved above, that *ſtratum* of ſand-ſtone, which occurs before they come to the fuller's-earth, does not lie, as in coal-pits, immediately over the matter they dig for, like a cieling; but even in the middle of the ſuperjacent *ſtrata* of ſand, and therefore can be no ſecurity to them, if they undermine.

The perpendicular fiſſures are frequent, and the earth in the *ſtrata*, beſides its apparent diſtinction into layers, like all other kinds of matter, by reaſon of its peculiar unctuouſneſs, or the running of the adjacent ſand imperceptibly into it, breaks into pieces of different angles and ſizes.

As to the geographical ſituation of theſe pits, they are dug in that ridge of ſand-hills by *Woburn*, which near *Oxford* is called *Shotover*, on which lies *Newmarket-Heath* by *Cambridge*, and which extends itſelf from eaſt to weſt, every where, at about the diſtance of eight or ten miles from the *Chiltern* hills, which in *Cambridgeſhire* are called *Gog-Magog*; in *Bucks* and *Oxon*, the *Chiltern* hills, from the chalky matter of which they

they chiefly conſiſt; which two ridges you always paſs, in going from *London* to the north, north-eaſt, or north-weſt counties in the manner above-mentioned: After which you come into that vaſt vale, which makes the greater part of the midland counties of *Cambridge*, *Bedford*, *Bucks*, *Northampton*, *Oxford* and *Glouceſter*; and in which are the rivers *Cam*, *Ouſe*, *Nen*, *Avon*, *Iſis* and others: Which Mr. *Holloway* takes notice of, becauſe it confirms what Dr. *Woodward* ſays of the regular diſpoſition of the earth into like *ſtrata* or layers of matter, commonly thro' vaſt tracts; and hence Mr. *Holloway* queſtions, whether fullers earth may not probably, be found in other parts of the ſame ridge of ſand-hills, among other like matter.

An Invitation for making Meteorological Obſervations; *by Dr.* Jurin. Phil. Tranſ. N° 379. p. 422. *Tranſlated from the* Latin.

THE various temperaments of the air (we breathe in) as to heat and cold, or the various changes of moiſt and dry weather, eſpecially if great and ſudden, are juſtly reckoned to influence more or leſs the health of mankind: In making theſe obſervations, therefore, not only phyſicians, but other curious enquirers into nature have in all ages been at no ſmall pains: And in the laſt century, by the diligence and ingenuity of philoſophers; inſtruments and machines were invented, by which the ſeveral degrees and changes of the weight, heat, moiſture and elaſticity of the air, are not only exhibited to the eye, but brought to weight and meaſure.

Nor did they ſtop here, but they endeavoured to diſcover, as much as poſſible, the cauſes of theſe changes; for which purpoſe they carefully ſet down in diaries the obſervations, made with theſe new invented inſtruments, on the weight, moiſture and heat of the circumambient air, and to theſe they added ſeveral other obſervations, with reſpect to the weather, and face of the ſky, the winds and quantity of rain, as may be ſeen in the *Philoſophical Tranſactions* and elſewhere.

Perhaps it is not eaſy finding a better method of obſervation, than that juſt now mentioned; and were there a competent number of obſervers ſtationed in convenient places; and ſhould ſome one compare all their diaries together to obſerve wherein

wherein they agreed or differ'd from each other, we ſhould then have ſuch a hiſtory of the air, as now we can hardly expect.

For, it is certain, that generally the ſudden changes of the weather are chiefly to be attributed to the winds; and when we come to know (by ſuch a method of obſervation, as that above-mentioned) in what places they roſe, what courſe they kept, at what time, and thro' what tracts of the earth they paſſed over; by this means, probably, a way might be opened for diſcovering the origin and cauſes of the winds: This one thing at leaſt, (which would be of no ſmall conſequence in theſe diſquiſitions, and which now is generally reckoned a probable conjecture) would be gain'd by it, *viz.* we ſhould be enabled by accurate obſervations to find out either the truth or falſhood of Dr. *Halley*'s opinion in *Phil. Tranſ.* N° 181, who thinks that the aſcent of the mercury in the barometer is owing to the winds blowing towards the ſame place from contrary points, and ſo collecting the air and accumulating it, as it were; as on the contrary that its deſcent is cauſed by the winds carrying the air from the ſame place towards contrary parts, and thus exhauſting it, as it were.

The curious, therefore, (who would contribute to improve this part of natural hiſtory) are deſir'd to mark in their diary, once a day at leaſt, the height of the barometer and thermometer, the point the wind blows from, with ſome account of its degree of ſtrength, the face of the heavens and the quantity of rain or ſnow, fallen ſince the laſt obſervation: And ſhould any one be pleas'd to add his obſervations, made with a hygroſcope or with a magnetical needle, it would not be at all unacceptable.

It would be proper as often as any violent ſtorm happens, accurately to ſet down its riſe, increaſe, greateſt violence, its abating and period, as alſo the reſpective heights of the barometer.

Such as know the method of filling and making a barometer, may uſe the common or open one, as it is call'd; let the tube be the fourth at leaſt, or the third part of an inch in diameter, ſince the mercury is found to ſubſide in narrower tubes below its juſt height, vide *Phil. Tranſ.* N° 363, let the diameter of the ciſtern or veſſel containing the mercury be at leaſt 8 or 10 parts greater than the diameter of the tube;

tube; that when the mercury riſes or falls in the tube, its height in the ciſtern may continue invariable, or as nearly ſo as poſſible.

Such as chuſe to make uſe of a cloſe or portable barometer, may have them made by that ingenious artiſt Mr. *Francis Hauksbee* in *Crane-court*, *London*; who can furniſh them with accurate thermometers, graduated according to a ſcale, for many years known to the curious.

Such as make uſe of any other thermometer, are deſir'd to mark in their diary its ſite, ſtructure, the ſcale it is graduated by, as alſo the name of the workman: The Dr. takes the moſt convenient place for a thermometer to be a room towards the north, where a fire is never or at leaſt very rarely made.

That the diaries may be the more eaſily compared together, it will be proper to obſerve the following method.

Let the firſt column exhibit the day and hour of the obſervation: All the obſervers are deſir'd to make uſe of the *Julian* or old ſtile in their diaries.

The ſecond the height, to which the mercury riſes in the tube above the ſurface of that in the ciſtern, in inches (or the twelfth part of a *London* foot) and decimal parts of an inch; a *London* to a *Paris* foot is as 15 to 16 nearly.

The third, the degree, and the decimal parts of a degree, to which the ſpirits in the thermometer reach.

The fourth, the point the wind blows from and its degree of ſtrength, which laſt may always be ſhewn by ſome of the following numbers, 1, 2, 3, 4; 1 denoting the ſofteſt breath of air, that ſcarce moves the leaves of trees; 4 its greateſt degree of ſtrength; 2 and 3 the intermediate degrees thereof; and 0 a perfect calm.

The 5th, the face of the heavens, with a ſhort account of the weather.

The 6th and laſt, the height of rain or diſſolv'd ſnow, fallen ſince the laſt obſervation, in *London* inches and decimal parts of an inch.

This may be eaſily eſtimated by means of a funnel, two or three foot wide, another veſſel receiving the water from the funnel, and a cylindrical meaſure with a gage divided into inches and decimal parts of an inch: Let the ſituation of the funnel be ſuch, that whatever wind blows, no part of the rain may be intercepted, either by the intervention of the houſe or any other impediment. Let the veſſel containing

the

the water be close shut every way that none of it may evaporate, with only a narrow hole to receive the water from the funnel above: Moreover, let the diameter of the cylindrical measure be 10 parts less than that of the funnel; so that by this means an inch of water in the measure may be estimated to have fallen to the height of the hundredth part of an inch in the funnel, and consequently, on the rest of the earth; and in like manner for decimal parts of an inch.

At the end of every month and year let the mean height of the barometer and thermometer in each be subjoined; as also the sum of all the heights of the rain, fallen in the whole month or year: The said mean height will be obtained, by bringing into one sum all the observations of the heights of the barometer, made in the morning, and the observations of the thermometer either in the morning or the greatest that day (which happens about three or four o'clock in the afternoon) and dividing that sum by the number of days.

All, who are pleased to make the abovementioned observations, either in whole or in part, are desired to transmit copies of the diaries, continued to the end of each year, to the secretaries of the *Royal Society*; to the end they may be compared with the diary, made at *London*, by the said *Society*'s order; and it is proposed, that whatever can be gathered from comparing these diaries together, shall be published every year in the *Philosophical Transactions*.

The

The form of the diary

Day and hour 1723 Nov. O. S.	Height Barom. inches tenths	Height Therm. deg. tenths	winds	Weather	Rain inches tenths
1. 8 a. m.	29 . 75	49 . 6	S. W. 1	The sky overcaſt	0 . 035
4 p. m.	29 . 56	47 . 3	S. W. 2	Showers at intervals Sunſhine at times	0 . 043
2. 7 ½ a. m.	29 . 24	48 . 5	S. 1	Almoſt conſtant rain	0 . 725
3. 9 a. m.	29 . 95	49 . 7	N. 1	Clear sky	0 . 032
5 p. m.	30 . 4	49 . 2	N. 1	Clear sky	0 . 000
4. 7 a. m.	29 . 9	47 . 0	S. W. 1	Scattered clouds	0 . 000
10	29 . 7	46 . 2	S. W. 2	Showers at times	0 . 103
12	29 . 4	45 . 0	S. 3	The sky almoſt quite covered with clouds	0 . 050
3 p. m.	28 . 8	46 . 0	S. 4	Scattered clouds	0 . 000
5	28 . 6	47 . 2	S. W. 4	The ſame face of the sky	0 . 000
7	28 . 9	48 . 0	S. W. 2	Rain	0 . 000
9	28 . 9	48 2	0	Almoſt conſtant rain	0 . 305
5. 7 a. m	29 . 7	53 . 4	N. E. 1	Clear sky froſt	0 . 250

An Account of Stones *voided* per anum; *by Mr.* David Martineau. Phil. Tranſ. N° 380. p. 433.

THE ſtones, repreſented by Fig. 3, 4, 5, 6, 7. Plate XI. were voided by a woman by the *anus*, on the 26, 27 and 28. of *March* 1723, who was then, according to computation, about the 11th or 12th week gone with child: *March* 23. Mr. *Martineau* was called in the evening to this woman, who in appearance was in the extremity of a convulſion fit, attended with violent vomitings, after which, ſhee complained of great pain in her back, from her reins downwards to the *anus*; upon which he blooded her, order'd ſomee anti-emetics and left her: She continued in great pain all thee day; on the 24. in the evening her fits returned again with double force, her pains alſo increaſing like labour pains, put Mr. *Martineau* upon farther enquiry, but nothing appeared more than ordinary: The extremity of pain the patient was in put him on uſing glyſters; but none could be thrown up: upon which he gave her a gentle draught, which ſhe obſerv'd increaſed her pain with a ſtrong *teneſmus*, that continued neai three hours, before the largeſt ſtone Fig. 3. appeared, which ſtopped at the upper part A D, but was quickly removed by preſſing the lower protuberance C; upon which, with a plentiful diſcharge, ſhe had eaſe, ſlept ſome hours, waking with a deſire to go to ſtool, and with it ſhe voided two more, repreſented Fig. 4, 5; on the next day at 4 hours interval thee other 2, repreſented Fig. 6, 7. The patient recovered perfectly, and on the 24th of *Auguſt* was delivered of a living female child. Upon enquiry, Mr. *Martineau* found, ſhe had been frequently troubled for 14 years, with pains in her ſide and ſtomach without vomitings.

The largeneſs of the ſtones, their ſeat and ſubſtance, is what ſeemed remarkable to Mr. *Martineau*; they were all alike in colour and weight, according to their dimenſions.

	oz.	*p.w.*	*gr.*		*inch.*			*inch*
Fig. 3.	2	16	22	A B C D	8		A D the top of the	6
Fig. 4.		8	12	E F G H	5 $\frac{1}{8}$		E G ſtone oval.	4
Fig. 5.		7	3	I K L M	5		I L	3
Fig. 6.		7	12	N O P Q	4 $\frac{8}{8}$		N P	3
Fig. 7.		7	12	R S T U	4 $\frac{1}{8}$		R T	4

In the laſt figure 1 repreſents a cruſt taken off the ſtone at the extremity; 2 the ſtone and cruſt ſoft like a cheſnut and reſembling wool; upon cutting into the ſtone at 3, it was found hard.

Of the Globules *in the* Blood *and* Lees *of* Wine; *by M.* Leewenhoeck. Phil. Tranſ. N° 380. p. 436. *Tranſlated from the* Latin.

BY a letter from Dr. *Jurin* to M. *Leewenhoeck*, it appears that the former found his obſervations about the ſize of the blood-globules agreeing with thoſe made by the latter; and moreover, that he thought this diſquiſition not indifferent or uſeleſs; ſince it was not impoſſible, but that ſuch an obſervation of the ſize of the blood-globules in different animals, together with their ſmall veſſels, might ſome time or other lead us to find out where and by what means theſe globules are form'd, and hence retain a determinate thickneſs in the ſame animal: Laſt of all, the Dr. ſays, that this well deſerved farther enquiry.

To this M. *Leewenhoeck* objects, that he is apt to think, we ſhall never come to the knowledge how theſe globules are coagulated from a fluid matter into other globules of a determinate bigneſs.

M. *Leewenhoeck* procured wine on the fret, becauſe it produces globules, almoſt equal to thoſe of the blood, and which we call the lees; and tho' he viewed this wine thro' a microſcope, yet he obſerved nothing other than a vaſt number of exceeding ſmall air-globules, riſing up to the ſurface, and carrying along with them thoſe globules, called the lees; as ſoon as they reached the ſurface of the wine, they were ſeparated from the air-globules and ſubſided, without being able to diſcover, how the globules, we call the lees, were formed: and as ſoon as theſe globules ſubſided, another air-globule proceeded from each of them, that mounted up again to the ſurface.

M. *Leewenhoeck* imagines, that that coagulation and determinate ſize of the blood-globules was originally ſo: For, if one globule were bigger than another, the conſequence would be an irregular circulation; becauſe he had obſerved the blood flow with rapidity thro' various blood-veſſels ſo fine, as only to admit a ſingle globule to paſs thro' them at a time.

Of the Generation of Animals *and of the Palpitation of the* Diaphragm; *by M.* Leewenhoeck. Phil. Tranſ. N° 380. p. 438. *Tranſlated from the* Latin.

DR. *Jurin* in a letter to M. *Leewenhoeck* acquainted him, that ſome anatomiſts attempted to write a new ſyſtem upon his notion of generation, affirming, that the *ovum* is the proper *nidus* in which the *animalculum* of the *ſemen maſculinum* is lodged, and that the ſame *ovum* being thereby impregnated, is afterwards conveyed thro' the *tuba Fallopiana* from the *ovarium* into the *uterus*.

M. *Leibnitz* a little before his death, wrote a letter to M. *Leewenhoeck* from *Hanover*, acquainting him, that he embraced his opinion on generation, and demonſtrated it in a book, he publiſhed; but that a Gentleman in *Italy* was about publiſhing ſomething againſt this ſyſtem of generation; and that another in *Germany* told him, that the whole *ovarium* was found in the *tuba Fallopiana*; and that there were profeſſors in *Germany*, who denied the exiſtence of any *animalcula* in the *ſemen maſculinum*.

Yet notwithſtanding all the contradictions M. *Leewenhoeck* had already or would hereafter ſuffer, he propoſed to continue in his opinion about generation; becauſe, he found *animalcula* in all ſorts of ſeeds, not excepting the various ſpecies of birds and fiſhes.

Moreover, we find in all the ſeeds of trees and plants, even the ſmalleſt (provided they are not impalpable) that the plant is formed, and which he formerly compared with the *animalcula* in the *ſemen maſculinum*; and the mealy matter in ſeeds is deſtined for nouriſhing theſe little plants within them, till itſelf throws out its roots and can be nouriſhed from the earth.

M. *Leewenhoeck* could not conceive, how any ſhould attempt to lay down and publiſh ſuch idle poſitions, *viz.* that the *animalcula* of the *ſemen maſculinum*, laid in the *uterus*, or *tuba Fallopiana* ſhould afterwards be conveyed thro' the *tuba Fallopiana* from the *ovarium* into the *uterus*. Theſe poſitions are ſo much without any foundation, as to deſerve no farther anſwer.

M. *Leewenhoeck* was perſuaded that the diaphragm is ſo large an inſtrument, as to put the whole *abdomen* into a continual motion, whereby the food in the ſtomach and inteſtines is comminuted in ſuch a manner, as to be reduced into a fluid

luid matter, fit to enter theſe ſmall blood-veſſels, ſo numeous in the cavities of the inteſtines, not as ſome would perſuade us, thro' the orifices; but as M. *Leewenhoeck* thinks thro' he fine coats of theſe exceeding ſmall blood-veſſels.

Now, if we ſuppoſe, that an artery in a well conſtituted body has 3600 pulſations in an hour, and that four pulſations are performed in the time of one reſpiration, there will hence be computed 900 reſpirations every hour, and ſo often will the ſtomach and inteſtines be compreſſed.

M. *Leewenhoeck* felt ſeveral ſlow palpitations in his diaphragm; wherefore, he thought of a glaſs-tube, he had invented, to convey the ſpirits of a certain liquor into his breaſt, in order to try, whether that uncommon motion of the diaphragm could be removed.

He therefore filled the tube to the height of three fingers with ſpirits of wine, in which were infuſed for a long time the following aromatics, *viz.* nutmeg, mace, a little clove, a deal of cinnamon and ſaffron; he then ſtrongly ſuck'd into his lungs the air that paſſed thro' the ſpirits of wine, by which means he expanded his diaphragm (which is a ſtrong membrane) pretty much, in order to try, whether he could thereby lay the palpitation or uncommon motion of the diaphragm; which ſucceeded for that time: But afterwards it happened, that this experiment (tho' he had drawn in the air very ſtrongly) did not anſwer his expectation.

M. *Leewenhoeck* rather inclines to Dr. *Jurin*'s opinion, *viz.* that the palpitation of the diaphragm is better grounded than that of the heart.

A Botanical Deſcription of the Flower and Seed Veſſels of the Plant, called Crocus autumnalis ſativus, *that produces the true* Engliſh Saffron *of the Shops; by Dr.* James Douglas. Phil. Tranſ. N° 380. p. 441.

THE flower of this curious plant is of the lilly kind, monopetalous, *infundibuliformis*, without any *calyx* or *perianthium*; its long fiſtulous beginning being afterwards expanded into ſix beautiful oblong ſegments, as repreſented at A A Fig. 8. Plate X.

It conſiſts of the following parts, *viz.* the *petalum*, the *ſtamina*, the *apices*, the *ovarium*, and the *ſtylus*, with its *capillamenta tubiformia*.

The *petalum* is diſtinguiſhed into a long, hollow or fiſtulous part, which lies incloſed within the common and proper

involucra

involucra of the plant, arising from the top of the *ovarium* or seed vessel, and six segments, as represented at *a a*.

While this narrow, tubulous part runs between the leaves and integuments that surround them, it is of a white colour; but as soon as it is disengaged from these, it insensibly acquires a purple colour, which, a little before its division, as it begins to be enlarged, and grow more open, inclines to a red.

This narrow fistulous part of the flower, about 1 or 2 inches above the *theca communis*, forms six foliaceous segments, or divides into so many oblong, purple coloured *petala*, as represented *b c*.

Three of these flower-leaves are larger than the other three; but in all other respects are much alike, as at *c c c*.

The length of the largest is from 1 inch $\frac{3}{4}$, to two inches; the breadth seldom above $\frac{1}{2}$ an inch: The shorter leaves are from 1 inch, to 1 inch and $\frac{3}{4}$ in length; their breadth being something less in proportion.

The inside of each *petalum* is of a violet-purple colour, vein'd with a few small lines, of a deeper dye, running lengthways, intermix'd with white; or the whole is beautifully checquered with blue and white colours.

The outside is of a lighter blue, with several whitish risings or ridges; and just at the bottom of the leaf it is of a deeper blue: The three small leaves *b b b*, are much of the same colour, only the purple seems to be somewhat deeper.

The number of the *petala* is for the most part 6: Yet in some flowers we may observe 7 or 8, but then they are not so large, as when they are fewer.

In each flower are three *stamina* or chives, *d d d*, properly so called, arising from the inner surface of the tubular part of the flower, just before its division into the *petala*, where they make, for some space, an appparent ridge; and then they stand upright, opposite to the three large leaves; they are of a whitish colour, inclining to a light purple, being but little above $\frac{1}{4}$ of an inch in length.

In those plants that have seven or more *petala*, the number of the *stamina* is likewise increased to five or more.

Each of these *stamina* has its proper *apex*, *e e e*, which is a peculiar oblong substance, standing upright, opposite to the larger flower-leaves, of a yellow colour, fork'd, at the lower end, where it is not unlike the tongue of a small bird, fixed to the *stamen*; here likewise it is broadest; but as it ascends

aſcends it becomes narrower and its upper extremity is a little crooked, or turned to one ſide, it ſeldom exceeds ½ an inch in length.

It appears as if it were double with a longitudinal furrow in the middle, in which hollow the *ſtamen* ſeems to be faintly continued for ſome ſpace.

When the plant is full grown, they are all loaded with that kind of ſmall duſt, called *farina fœcundans*.

The *ovarium*, called likewiſe the *vaſculum ſeminale* and *piſtillum* by ſome, as *f f*, is a particular body, which ariſes from the top of the *pedunculus g*, about ¼ of an inch long or a little more, of a deep white colour, three cornered and divided into three *loculamenta* or *capſulæ*, in which the ſeeds (which however ſeldom come to perfection with us) are form'd, growing bigger and bigger, after the flower falls off; nay, even in this blooming ſtate, if you cut this veſſel a-croſs the middle, you may perceive the whitiſh rudiments of the ſeeds.

From the upper part or *apex* of the *ovarium* ariſes the *ſtylus h*, a long ſlender tube, that lies incloſed within the tubulous or fiſtular portion of the flower, being there of a whitiſh colour, but changing into a yellow before its diviſion.

This *ſtylus* ordinarily ſplits into three parts, juſt oppoſite to the top of the *ſtamina*, where the *apices* take their riſe *i i i*, and thus far it ſtands upright in the middle of the *ſtamina*; for the ſake of which only, this plant is cultivated, and being prepared (as ſhall be deſcribed anon) makes the true ſaffron of the ſhops, ſo frequently uſed in phyſic.

The Dr. calls theſe parts of the *ſtylus*, from their figure and ſhape, *capillamenta tubiformia* or *appendices ſtyli ſalpingoeides*, in as much as they exactly repreſent a trumpet, and are not unlike the *tuba Fallopiana* in women, being narrower at their origin and growing gradually larger towards the other extremity; which, like the common trumpet, is open and expanded; and like the *tubæ Fallopianæ*, is jagged or fringed at the edge, as *k k k*: It may likewiſe be very properly called *crocus officinarum*, becauſe that is the only part that is uſed in the ſhops.

They are of a yellow colour, juſt at their beginning, continued from the upper part of the *ſtylus*, but afterwards they are all of a deep red colour, only their jagged extremities are tipt with a white, inclining to a yellow.

Thoſe *tubæ* or *capillamenta* are from 1 to 1 inch and ½ in length.

The

The *ſtylus* while undivided, is ſtrong enough to ſupport itſelf, being incloſed within the tubular part of the flower; but the *capillamenta*, being very weak and ſlender at their beginning, hang down between the *petala*.

Fig. 8. Plate X. repreſents a root of the ſaffron plant, that has two ſtalks incloſed in one common *vagina*; the whole flower with the *ſtamina* and *apices* in one, and the *ſtylus* only in the other; with the leaves, *pedunculi* and *ovarium* in both, being fairly delineated from the life; A A the flower expanded into ſix beautiful ſegments; *a a* the fiſtulous part of the flower; *b c* the ſix *petala*; *c c c* the three larger *petala*; *b b b* the three ſmaller ones; *d d* the three *ſtamina* or chives; *e e e* the three *apices*; *f f* the *vaſculum ſeminale*; *g g* the *pedunculi*; *h h* the *ſtylus*; *i i i* the three *capillamenta tubiformia*; *k k k* the jagged extremity of the *capillamenta*; B B the root; C C the graſſy leaves.

An Account of M. Leewenhoeck's *curious* Microſcopes, *preſented to the* Royal Society; *by Mr.* Folkes. Phil. Tranſ. N° 380. p. 446.

IT is upwards of 50 years, ſince the late M. *Leewenhoeck* firſt began his correſpondence with the *Royal Society*; when he was recommended by Dr. *Regnerus de Graaf*, as a perſon already conſiderable by his microſcopical diſcoveries, made with glaſſes, contrived by himſelf, and excelling even thoſe of the famous *Euſtachio Divini*, ſo much talked of in the learned world; and as he had ever ſince that time applied himſelf, with the greateſt diligence and ſucceſs, to the ſame ſort of obſervations, no doubt can be made of the excellency of thoſe inſtruments he had ſo long uſed, ſo much improved, and upon the fulleſt experience ſo often commended in his letters; a great part of which, at his deceaſe, he bequeathed to the *Royal Society*.

This legacy conſiſts of a ſmall *Indian* cabinet, in the drawers of which are 13 little boxes or caſes, each containing two microſcopes, handſomely fitted up in ſilver; all which, not only the glaſſes, but alſo the *apparatus* for managing of them, were made with M. *Leewenhoeck*'s own hands: Beſides which, they ſeem to have been put in order in the cabinet by himſelf, as he deſigned them to be preſented to the *Royal Society*, each microſcope having had an object placed before it, and the whole being accompanied with a regiſter of the ſame in his own hand writing; as being deſirous the Gentlemen of the *Society* ſhould, without

without trouble, be enabled to examine many of thoſe objects on which he had made the moſt conſiderable diſcoveries, and which in a particular manner deſerve their attention.

The 13 caſes abovementioned are numbered from 15 to 27 incluſive, correſponding to which is the regiſter of the objects, two to every caſe, as follows.

N° 15. Globules of blood, from which its redneſs proceeds.
A thin ſlice of lime-tree wood, where the veſſels, conveying the ſap, are cut tranſverſely.

N° 16.
The eye of a gnat.

N° 17. A crooked hair, to which adheres a ring-worm, with a piece of the *cuticula*.
A ſmall hair from the hand, by which it appears, that thoſe hairs are not round.

N° 18. A piece of the cod-fiſh (*cabeljaeuw*) ſhewing how the fibres lie oblique to the membranes.
An embryo of cochineal taken from the egg, in which the limbs and horns are plainly to be ſeen.

N° 19. Small pipes, which compoſe the tooth of an elephant.
Part of the cryſtalline humour from the eye of a whale.

N° 20. A thread of ſheep's wool, which is broken and ſeems to conſiſt of ſeveral leſſer threads.
The inſtrument, whence a ſpider ſpins the threads, that compoſe his web.

N° 21. A granade or ſpark made in ſtriking fire.
The veſſels in a leaf of tea.

N° 22. The *animalcula in ſemine maſculino* of a lamb, taken from the teſticle *July* 24, 1702.
A piece of the tongue of a hog, full of ſharp points.

N° 23. A fibre of codfiſh, conſiſting of long ſlender particles.
Another of the ſame.

N° 24. A filament, conveying nouriſhment to the nutmeg, cut tranſverſely.
Another piece of the ſame, in which may be ſeen the figure of the veſſels.

N° 25. Part of the abovementioned bone or tooth, conſiſting of hollow pipes.
An exceeding thin membrane, which covered a very ſmall muſcle.

N° 26. Veſſels, by which membranes receive nouriſhment and increaſe.
A bunch of hair from the inſect, called a hair-worm.
N° 27. The double ſilk, ſpun by the worm.
The eye of a fly.

It were endleſs to enter into any particulars of what is to be obſerved in any of theſe objects or give any account of M. *Leewenhoeck*'s diſcoveries; they are ſo numerous as to make up a conſiderable part of the *Philoſophical Tranſactions*, and when collected together to fill four pretty large volumes in quarto, which have been publiſhed by himſelf at ſeveral times: And of ſuch conſequence, as to have opened entirely new ſcenes in ſome parts of *Natural Philoſophy*, particularly, that famous diſcovery of the *animalcula in ſemine maſculino*, which has given a perfectly new turn to the theory of generation, in almoſt all the authors that have ſince wrote upon that ſubject.

As to the conſtruction of theſe inſtruments; it is the ſame in them all, and the *apparatus* is very ſimple and convenient: They are all ſingle microſcopes, conſiſting each of a very ſmall double convex glaſs, let into a ſocket, between two ſilver plates, rivetted together, and pierced with a ſmall hole; the object is placed on a ſilver point or needle, which by means of ſcrews of the ſame metal, provided for that purpoſe, may be turned about, raiſed or depreſſed, and brought nearer, or put farther from the glaſs, as the eye of the obſerver, the nature of the object, and the convenient examination of its ſeveral parts may require.

M. *Leewenhoeck* fixed his objects, if they were ſolid, to this ſilver point, with glue; and when fluid, or of ſuch a nature, as not to be commodiouſly viewed, unleſs ſpread upon glaſs, he firſt fitted them on a little plate of talc, or exceeding thin blown glaſs, which he afterwards glewed to the needle, in the ſame manner as his other objects.

It is true, the obſervation of the circulation of the blood and ſome others, require a ſomewhat different *apparatus*; and ſuch a one he had, to which he occaſionally fixed theſe ſame microſcopes: But as it makes no part of this cabinet, Mr. *Folkes* only takes notice, that it may be ſeen in a letter to the *Royal Society*, dated the 12. of *January*, 1689. and publiſhed in his *Arcana naturæ detecta* N° 69. and this may ſerve to ſhew the univerſal uſe of theſe microſcopes, and (among other things) induced

induced Mr. *Folkes* to believe, these were the kind of microscopes, generally, if not solely, used by this curious Gentleman in all his observations, and to which we are obliged for his most surprising discoveries.

Upon the late Queen *Mary*'s visiting M. *Leewenhoeck* at *Delft*, and viewing his curiosities with great satisfaction, he presented her with a couple of his microscopes, which, as Mr. *Folkes* was informed by one who had them a considerable time in his custody, were of the same sort with these, and did no ways differ from any of the 13 cases, contained in the drawers of this cabinet.

The glasses are all exceeding clear, and shew the object very bright and distinct, which must be owing to the great care this Gentleman took, in the choice of his glass, his exactness in giving it the true figure; and afterwards, amongst several, reserving only such for his own use, as he, upon trial, found to be most excellent. Their powers of magnifying are different, as different sorts of objects may require; and as on the one hand, being all ground glasses, none of them are so small; and consequently, magnify to so great a degree, as some of those drops, frequently used in other microscopes: Yet on the other hand, the distinctness of these very much exceeds what Mr. *Folkes* had met with in the glasses of that sort; and this was what M. *Leewenhoeck* ever principally proposed to himself, rejecting all those degrees of magnifying, in which he could not so well obtain that end: For, he informs the *Royal Society* in one of his letters to them, where speaking of the excessive praise some give their glasses upon this account, that tho' he had had upwards of 40 years glasses by him of an extraordinary smallness, he had made but very little use of them; as having found, in a long course of experience, that the most considerable discoveries were to be made with such glasses, as magnifying but moderately, exhibited the object with the most perfect brightness and distinctness.

But however excellent these glasses may be judged, M. *Leewenhoeck*'s discoveries are not entirely to be attributed to their goodness only; his own great judgment and experience in the manner of using them; together with the continual application, and indefatigable industry with which he often and long viewed the same subject, and that under many and different circumstances, cannot but have enabled him to form better judgments of the nature of his objects, and see farther into their constitu-

tion, than it can be imagined any other perſon can do, that neither had the experience, nor had taken the pains this curious author had ſo long done.

Nor ought we to forget a piece of ſkill, in which he very particularly excelled, which was that of preparing his objects in the beſt manner, to be viewed by the microſcope; and of this any one will be ſatisfied, who ſhall apply himſelf to the examination of ſome of the ſame objects, as do ſtill remain before theſe glaſſes; at leaſt Mr. *Folkes* found ſo much difficulty in this particular, as to obſerve a very ſenſible difference between the appearances of the ſame object, when applied by himſelf and when prepared by M. *Leewenhoeck*, tho' viewed with glaſſes of the very ſame goodneſs.

Mr. *Folkes* the rather inſiſts on this, as it may be a caution, not raſhly to condemn any of M. *Leewenhoeck*'s obſervations, tho' even with his own glaſſes, we ſhould not immediately be able to verify them ourſelves. We muſt be under very great diſadvantages for want of the experience he had; and M. *Leewenhoeck* himſelf has put us in mind more than once, that ſuch as are the beſt ſkilled in the uſe of magnifying glaſſes may be miſled, if they give too ſudden a judgment upon what they ſee, or till they have been aſſured from repeated experiments. But we have ſeen ſo many and thoſe of his moſt ſurpriſing diſcoveries ſo perfectly confirmed, by great numbers of the moſt curious and judicious obſervers, that there ſurely can be no reaſon to diſtruſt his accuracy in thoſe others, which have not hitherto been ſo frequently or carefully examined.

The

The Bills *of* Mortality, *&c. of ſeveral conſiderable Towns in* Europe, *from* Chriſtmas 1716, *to* Chriſtmas 1717, *extracted from the* Acta Breſlavienſia; *by Dr.* Sprengell; Phil. Tranſ. N° 380. p. 454.

Breſlaw in *Sileſia.*

Buried		Chriſtened	
In *January*	91	Males	584
February	113	Females	576
March	136		1160
April	108	Among the Buried were.	
May	127	Married men	226
June	144	Married women	144
July	144	Widows and widowers	157
Auguſt	208	Batchelors	60
September	128	Maidens	57
October	96	Boys and girls under ten years old,	419
November	113		397
December, only till the 24th,	77	Still-born Boys	37
		Still-born Girls	17
	1485		1514

N. B. Here in the ſum total are 29 more than in the number of the buried above, by reaſon of ſeveral ſtill-born children that had no regular burial, and ſo were not reckoned.

In the imperial town *Vienna* there died.

Young people	3179
Old	2026
Total	5205

Among which were,

Of 90 years old	2	Of 99	2
91	1	100	1
92	3	102	2
93	1	103	1
94	3	104	1
95	2	115	1
96	3		

Born and chriſtened 4030 children.

In *Dreſden*, *Saxony*, died 1908

Chriſtened 1443

In

In *Dantzick* died 1605
Chriſtened 2102

In *Eſperies*, or *Eperies*, alias *Epperies*, a town in *Upp*
Hungary, died 132
Chriſtened 157
Moſt of theſe died of the ſmall-pox

The Bills *of* Mortality *from* Chriſtmas 1717 *to* Chriſtm
1718.

Breſlaw.

Buried.		Chriſtened.	
From the 25th of *Dec.* to the 31ſt.	21	Males	55
January	129	Females	55
February	112		11
March	108		
April	98	Among the dead were.	
May	88	Married men	23
June	79	—— Women	14
July	86	Widows and widowers	12
Auguſt	118	Batchelors	6
September	98	Virgins	6
October	119	Children to 10 years of age, Boys	28
November	100	Children to 10 years of age, Girls	28
December, till the 24th,	81	Stillborn Boys	47
		Stillborn Girls	25
Total	1255		1255

In *Vienna* died	Men	1432	Women	1129
	Boys	1844	Girls	1705

In all 6110

Among which were froze to death in the ſtreet	2	Trod to death by horſes	4
Murder'd	2	Drowned	5
Kill'd by a waggon	2	Beat to death	1
By falls from houſes, *&c.*	11	Smother'd in a houſe of office	4
Kill'd with ſwords, knives, and ſabres,	9	Cut their throats	2
By taking wrong phyſick	1	Kill'd by the fall of houſes	5

Among

Among the dead were found

3 Persons of 90 years old.
1 92
2 93
3 94
3 95
1 96
2 97
2 98
2 99
1 105

Christened

Males	2185
Females	2057
Total	4242

Among which were 48 pair of twins, and once *Tergemini*.

In *Ratisbon* died of the *Lutherans* 235 persons; amongst which were 117 children, 13 young men, and 11 young women.

The following list exhibits an account of several cities in *Saxony*.

	Christened	*Bastards.*	*Buried*	*Pair married.*	*Communicants.*
In *Dresden*	1578	99	1412	501	78875
Wittemberg	286	16	317	61	13536
Leipzick	861	68	953	303	
Torgau	136	9	148	54	7917
Freyberg	340	13	373	104	24098
Stollberg	88		112	27	7314
Pulsnitz	73		75	39	4381
Konigsbruck	67		95	16	3351
Elstra	63		45	11	3021
Bautzen	207	5	135	55	14520
Annaberg	146	19	112	37	8426
Chemnitz	173	4	166	38	10690
Oschatz	103	3	82	20	5169
Altenburg	141		248	57	12901
Eulenberg	96	5	86	41	6194
Pirna	138	2	133	43	9164
Marienberg	91		194	18	5903
Georgenstad	182		191	36	3580
Harta, near *Waldheim*	40	3	20	29	3291

A list for a whole century, *viz.* from 1617 to 1717 inclusive, giving an account of the numbers of people married, christened, buried and number of communicants in the electoral

electoral city of *Freyberg*, near the banks of the *Molde* i
Saxony, Lat. 51. Long. 32° 43′ about 8 miles S. W. from *Dresden*

Numb. Years	*Pair married*	*Christened.*	*Bastards.*	*Buried*	*Communicants.*
1617	89	539	4	430	10530
18	122	423	10	384	9631
19	124	495	13	371	9789
20	97	428	10	451	10093
21	106	430	6	507	9676
22	110	380	7	517	9536
23	100	357	8	589	9206
24	96	383	4	417	9509
25	98	375	7	454	10058
26	78	381	6	549	11642
27	108	372	7	331	11027
28	82	371	4	318	10745
29	87	412	19	536	11531
30	80	330	7	1114	11180
1631	135	353	4	284	10524
32	88	425	5	1343	11326
33	174	163	4	879	9131
34	170	375	1	175	7447
35	87	285	9	231	7323
36	61	299	11	181	7590
37	68	386	3	383	7723
38	53	262	9	151	7682
39	45	292	4	354	9609
40	65	268	6	173	7594
1641	61	226	2	183	7562
42	48	309	4	193	8128
43	60	383	7	340	7770
44	65	231	7	156	7699
45	62	196	5	163	7050
46	57	230	6	187	7263
47	59	201	2	136	7173
48	59	197	1	182	7698
49	44	222	5	201	7739
50	65	199	1	162	7639

1651

Numb. Years.	Pair married	Christened.	Bastards.	Buried.	Commu-nicants
1651	65	232	1	168	8148
52	50	190	2	202	7948
53	62	208	2	215	8342
54	62	234	3	211	8648
55	73	202	1	194	6351
56	60	237	5	151	8493
57	58	230		174	8730
58	61	198	4	184	8561
59	57	251	3	177	8431
60	66	230		240	8856
1661	47	225	3	217	8962
62	53	203	2	206	9423
63	47	206	2	247	9264
64	48	219	2	179	9279
65	61	213		207	9406
66	65	218	4	216	9979
67	79	223		306	9448
68	63	228	5	232	9588
69	55	232		204	9572
70	70	218	4	206	9564
1671	65	237	3	291	10080
72	64	207	2	286	9221
73	69	234	1	215	9912
74	55	228		204	9678
75	60	233	1	246	10093
76	58	226	1	277	9956
77	80	240	3	296	10462
78	70	256	4	302	10422
79	91	225	4	254	10356
80	63	292	4	267	11349

Numb. Years.	Pair married	Christened.	Bastards.	Buried	Communicants.
1681	87	285	5	218	11043
82	69	282	7	253	10894
83	77	315	8	333	11054
84	78	334	6	314	10623
85	73	249	4	251	11070
86	73	282	5	243	11488
87	80	327	6	292	11542
88	88	308	11	234	11300
89	59	276	4	301	11393
90	72	290	5	310	11380
1691	72	269	6	289	11724
92	58	284	7	296	12679
93	72	316	10	253	11597
94	65	236	7	355	12533
95	57	239	6	339	13196
96	85	277	10	290	14900
97	76	216	4	180	15619
98	73	274	7	218	15677
99	64	264	5	258	16155
1700	74	251	7	310	18569
1701	81	283	10	188	19282
2	88	272	2	239	20383
3	95	271	6	241	20494
4	88	319	11	273	20837
5	72	295	12	357	20743
6	85	332	10	245	21813
7	82	310	18	378	21367
8	83	310	17	288	21838
9	71	302	10	305	21708
10	83	313	15	283	21699

Numb. Years.	Pair married	Christened.	Bastards.	Buried.	Communicants.
1711	81	327	8	311	22334
12	82	310	9	325	22837
13	87	308	9	340	23627
14	70	349	6	237	23470
15	82	323	8	283	23273
16	105	317	14	361	23371
17	79	373	13	325	23904
Total	7546	28851	582	30295	1211761

Here it is to be obſerv'd, that in the years 1625, 26, 27, 32, 33, the plague raged very much; as alſo that in 1630 there died meerly of the plague 764, and in 1680, 103: Beſides that, amongſt the chriſtened in 1713, a girl was baptiz'd, who was born and bred in *Turky*, but from Chriſtian parents.

But in general it may be obſerv'd, that this place was more populous before the war than it is at preſent; tho' in this century it cannot but very much recover and increaſe.

A liſt receiv'd from *Berlin*, giving an account of all that were born, married and buried in *Pruſſia* in four years.

	Year.	Born.	Pair married.	Buried.
In the kingdom of *Pruſſia*	1715	19606	4571	12000
	16	20669	4530	12155
	17	21443	4743	12301
	18	20994	4287	11047
	Sum	82712	18331	47503

The following liſt contains the number of all that were born, married, and buried in the reſt of the King of *Pruſſia*'s dominions in *Germany*, &c. for four yerrs.

	Year.	Born.	Pair married.	Buried.
In the electorate of *Brandenburg*.	1715	14820	4368	11663
	16	15758	4995	13149
	17	16636	4539	10805
	18	17319	4657	12891
	Sum	64533	18559	48508

	Year.	Born	Pair married	Buried
In the *Newmarck.*	1715	5487	1683	4751
	16	4973	1010	4502
	17	7052	1850	4673
	18	6684	1882	5069
	Sum	24196	7425	19001
In the dutchy of *Magdeburg.*	1715	7185	2064	4982
	16	7225	2186	5602
	17	8746	2306	6291
	18	8325	1934	5744
	Sum	31481	8490	22619
The dutchy of *Cleve.*	1715	6772	1964	5066
	16	7313	2001	6221
	17	7582	2051	6055
	18	7542	1891	5865
	Sum	29209	7907	23207
The dutchy of *Pomerania.*	1715	7243	2190	5373
	16	7386	2407	5557
	17	8196	2108	5020
	18	7896	2138	6231
	Sum	30721	8843	22181
The principality of *Halberſtad.*	1715	2371	710	1834
	16	2567	759	2542
	17	2759	764	1961
	18	2527	715	2022
	Sum	10224	2948	8359
The county of *Hohenſteim.*	1715	528	164	406
	16	639	155	444
	17	644	148	449
	18	612	158	416
	Sum	2423	625	1715

	Year.	Born.	Pair married.	Buried.
The principality of *Minden.*	1715	1711	579	1618
	16	1788	564	1829
	17	1991	542	1419
	18	1832	552	1486
	Sum	7322	2237	6352
The city of *Meurs.*	1715	406	151	386
	16	439	161	331
	17	454	166	281
	18	470	170	368
	Sum	1769	648	1366
In *Geldern,*	1715			
	16			
	17	1990	497	1066
	18	2053	489	1064
	Sum	4043	986	2130
The county of *Ravensberg*	1715	2188	679	1987
	16	2410	716	2349
	17	2312	672	1817
	18	2344	641	1951
	Sum	9254	2708	8104
The county of *Tecklenberg.*	1715	342	153	513
	16	553	163	543
	17	505	186	370
	18	507	135	387
	Sum	1907	637	1813
The county of *Lingen.*	1715	628	213	484
	16	699	154	638
	17	697	248	449
	18	706	221	840
	Sum	2730	836	2411

	Year.	*Born.*	*Pair married.*	*Buried.*
In *Lauenburg* and *Butow.*	1715	585	144	362
	16	587	169	420
	17	586	118	366
	18	554	153	321
	Sum	2312	584	1469
In the *French* colonies.	1715	—	—	—
	16	—	—	—
	17	607	181	564
	18	603	136	639
	Sum	1210	317	1203

The ſum total of all that were born in four years. } 306046

of all that were married 81881 pair.

of all that were buried 217941

More born than buried in number 88105

Here it is alſo to be obſerv'd, that in the year 1718, there died 84 perſons upwards of 91 years of age, and 32 upwards of 100, and one in the 116th year of his age: Beſides, in that very year there are reckoned 2088 baſtards.

A particular liſt, (receiv'd from the chancery of *Koningsberg*) of all that died in the year 1718, in the ſeveral borough towns in the kingdom of *Pruſſia.*

In the borough of	*Died*	*In the borough of*	*Died*
Brandenburg	581	Neudenberg *and* Saldau	330
Schacken	346	Lyck	141
Fiſhauſen	312	Oletzko	188
Taxium	456	Angerburg	182
Balga	392	Rhein	107
Preuſs Eylau	319	Orſelsburg	220
Bartenſtein	167	Johannisburg	141
Barthen	185	Lotzen	137
Oſterode *and* Hohenſtein	277	Neuhauſen	148
Seheſten	159	Labiau	249

In

In the borough of	*Died*	*In the borough of*	*Died*
Raſtenburg	275	Mummel	556
Preuſs Holland	264	Schonberg	126
Marungen *and* Liebſtadt	206	Gerdauen *and* Nordenburg	269
Marienwerder *and* Rieſenburg	393	Gilgenburg	109
Preuſs Marck	312	Teutſch Eylau	41
Tilſit	529	Neuhaff	15
Ragnit	381	*In the three cities of*	
Inſterburg, Georgenburg, Salau	808	Koningsberg	1756
		Sum	11077

In all theſe above-mentioned places were chriſtened 20994
Pair married 4287

	Died.	Chriſtened.	Married.
In *Dantzick* in the year 1718.	1579	1470	446 pair
In *Epperies*	123	173	

In the royal hoſpital at *Lisbon* out of 1251 foundlings, maintained by the King, there died 469, remained 782.

Experiments concerning the Degrees of Heat of boiling Liquors; *by M.* Fahrenheit. Phil. Tranſ. N° 381. p. 1. *Tranſlated from the* Latin.

M. *Fahrenheit*, finding in the Hiſtory of the Royal Academy of Sciences, that the celebrated M. *Amontons* had, by means of a thermometer of his own invention, diſcover'd, that water boils with a fixt degree of heat, was very deſirous of making ſuch another thermometer, in order to view with his own eyes this beautiful phænomenon of nature, and be convinced of the truth of the experiment.

M. *Fahrenheit* recollected what that ſagacious enquirer into nature, had writ about the manner of rectifying barometers; for, M. *Amontons* had obſerv'd, that the height of the column of mercury is ſomewhat, nay, ſenſibly affected by the different temperament of the mercury: From this M. *Fahrenheit* imagined, that perhaps, a thermometer might be made of mercury, whoſe ſtructure would not be ſo difficult, and yet by it he might diſcover the experiment he ſo much deſir'd.

Having

Having made ſuch a thermometer (tho' ſtill imperfect in ſeveral reſpects) the event anſwer'd his expectation to his no ſmall ſatisfaction.

The iſſue of the experiments is exhibited in the following table, the firſt column ſhews the ſeveral liquors that were made uſe of, the ſecond their reſpective ſpecific gravities; the third the degree of heat each liquor acquir'd by boiling.

Liquors.	The ſpecific gravity of liquors heated to 48 degrees.	The degrees of heat acquir'd by boiling.
Spirits of wine	8260	176
Rain water	10000	212
Spirit of nitre	12935	242
Lixivium of pot-aſh	15634	240
Oil of vitriol	18775	546

M. *Fahrenheit* thought it neceſſary to add the ſpecific gravity of each liquor, that if the experiments, either already made, or hereafter to be made by others, ſhould happen to differ from the above-mentioned, it might be known whether the difference were owing to the variation of the ſpecific gravity, or to other cauſes: Moreover, the experiments were made at different times, and hence likewiſe the liquors were affected with various degrees of heat; but becauſe their gravity is variouſly and unequally diſturbed, he reduced it to 48 degrees, which in his thermometer is the mean between the moſt intenſe artificial cold, made by the commixture of water, ice, ſal-armoniac, or even ſea-ſalt, and the degree of heat of the blood of a healthful perſon.

It is true, volatile oils begin to boil with any degree of heat; but their heat continually increaſes by boiling; the reaſon of which may, probably, be this, *viz.* that the more volatile particles fly off, while the reſinous ones, that have a greater attraction, remain behind.

But fix'd oils require ſo great a degree of heat, that the mercury in the thermometer begins to boil at the ſame time with them, and hence their degree of heat can ſcarce be found with certainty in the manner above-mentioned.

Excepting ſpirits of wine and water, probably, the degrees of heat in the other liquors, above-mentioned, may likewiſe vary; eſpecially, if they be taken in larger quantities, and boil ſome time.

Obſer-

Observations made in 1723, *on the* Barometer, Thermometer *and* Hygrometer, *&c. by M.* Nicolaus Cruquius. Phil. Tranſ. N° 381. p. 4. *Tranſlated from the* Latin.

IN 1723 M. *Cruquius* made the following accurate obſervations at *Leyden*, *Delft* and *Rheinburg*, on the mean height of the barometer and thermometer, as alſo on the mean variation of the hygrometer; the quantity of rain, dew, ſnow, hail, as alſo the quantity of water exhaled; and the height of the water in a well, out of which none had been drawn for a whole year; and on the monthly variation of a pocket-watch.

Barometer

Here M. *Cruquius* ſuppoſes the weight of the atmoſphere incumbent on the plane of a *Rheinland* ſquare foot; the weight of which atmoſphere is equal to 1947 *Amſterdam* pounds, when the height of the mercury in the barometer is 27 inches 7 lines; and 2094 *Amſterdam* pounds, when the height of the mercury is 29 inches 8 lines. This was obſerved to be the greateſt and leaſt degree of height of the mercury in the barometer for ſeveral years.

	pds.
January	2051
February	46
March	35
April	46
May	57
June	53
July	2044
Auguſt	46
September	54
October	55
November	53
December	35
Sum	575
12	
Mean weight	2048

Thermometer

Here he determined the heat of the atmoſphere in the place of obſervation from the rarefaction of the air, ſo that the greateſt degree of cold was obſerved, at 1000 degrees, pure water froze at 1070 degrees; and boiled at 1510.

	dgr.
	1076
	85
	102
	109
	126
	140
	1129
	141
	132
	121
	104
	96
	1361
	1113

Hygrometer

Here he meaſur'd the quantity of water in the atmoſphere in the place of obſervation according to the increaſe or decreaſe of the weight of a ſponge hung from a balance after firſt moiſtening it in a ſolution of ſal armoniac.

	weight
	81
	80
	80
	60
	57
	57
	58
	60
	61
	71
	77
	79
	821
	68

The quantity of water exhaled at *Rheinburg* into the open air in each month throughout the year 1723.

	lin.
	7
	14
	33
	36
	58
	57
	37
	39
	24
	15
	15
	12
	347
	28 inches 11 lines the whole year.

The height of rain, dew, snow and hail; particular care being taken that not the least quantity was lost by exhalation or any other way.		The height of the water in a well, measur'd from its brink to the surface of the water at the end of each month; from which no water was drawn all the time of the observation.	The variation of an accurate pocket-watch; shewing how many minutes it goes too fast or too slow every month; + denoting the increase and ÷ the decrease of its motion.
At *Delft*	At *Rheinburg*		
lin. decim.	*lin. decim.*	feet inch.	minutes.
— 17.9	— 21.2	— 5 : 4	— + 151 *Jan.*
— 25.1	— 23.8	— 4 : 11	— + 21 *Febr.*
— 18.8	— 28.	— 5 : 7	— ÷ 168 *Mar.*
— 5.5	— 7.5	— 6 : 10	— ÷ 120 *April*
— 4.2	— 2.7	— 8 : 1	— ÷ 123 *May*
— 3.2	— 4.8	— 9 : 3	— ÷ 130 *June*
— 38.6	— 28	— 9 : 7	— ÷ 90 *July*
— 41.9	— 40.2	— 9 : 7	— + 133 *Aug.*
— 15.1	— 14.8	— 9 : 9	— + 24 *Sept.*
— 8.2	— 11.3	— 9 : 8	— + 19 *Octob.*
— 30.7	— 29.7	— 9 : 3	— + 266 *Nov.*
— 30.6	— 40.	— 8 : 2	— + 252 *Dec.*
— 239.8	— 25.2	96 : —	+ 866, & ÷ 631 or + 235
At *Delft* 20 inches and at *Rheinburg* 21 inches for the whole year.		mean height 8 foot.	20′ too fast every month.

Obser

Observations made for a number of years.

Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	mean	sum	year
	lb.			BAROMETER									12	lb.	
1720	2034	34	38	36	38	42	45	36	50	40	38	23	454	2038	1720
1721	52	42	36	29	29	43	51	46	43	42	32	29	474	39	1721
1722	76	41	32	41	42	45	36	39	50	59	37	30	528	44	1722
1723	51	46	35	46	57	53	44	46	54	55	53	35	575	48	1723
	213	163	141	152	166	183	176	167	197	196	160	117		169	
4															
	2053	41	35	38	41	46	44	42	49	49	40	29		2042	
	deg.			THERMOMETER										dgr.	
1720	1087	89	86	102	126	126	145	138	121	107	91	91	1309	109	1720
1721	90	74	75	112	116	137	136	142	133	110	100	84	1309	109	1721
1722	79	90	97	109	122	134	139	140	135	117	101	90	1353	113	1722
1723	76	85	102	109	126	140	129	141	132	121	104	96	1361	113	1723
	332	338	360	432	490	537	549	561	521	455	396	361		444	
4															
	83	85	90	108	122	134	137	140	130	114	99	90		111	

Year	*Jan.*	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*	mean	sum	year
	weight			HYGROMETER									weight		
1721	89	82	73	80	69	64	63	68	76	76	89	88	917	76	1721
1722	88	85	76	63	62	62	63	68	70	76	81	88	882	74	1722
1723	81	80	80	60	57	57	58	60	61	71	77	79	821	69	1723
3	258	247	229	203	188	183	184	196	207	223	247	255		219	
	86	82	76	68	63	61	61	65	69	74	82	85		73	
	lines			Rain, *&c.* at *Delft.*									inch	lin.	
1715	11	20	36	8	15	18	95	62	36	37	47	15	400	33 : 4	1715
1716	19	20	14	7	17	4	48	19	55	57	21	32	313	26 : 1	1716
1717	31	15	29	29	31	28	29	28	24	32	29	28	333	27 : 9	1717
1718	21	18	6	30	17	17	35	27	14	46	21	25	277	23 : 1	1718
1719	33	20	10	11	17	4	12	34	23	25	24	22	235	19 : 7	1719
1720	36	24	21	21	15	20	23	55	47	56	25	20	363	30 : 3	1720
1721	20	31	27	59	30	34	15	41	27	57	30	48	419	44 : 11	1721
1722	2	20	25	23	15	22	49	53	25	7	21	53	315	26 : 3	1722
1723	18	25	19	5	4	3	39	42	15	8	31	31	240	20 :	1723
9	191	193	187	193	161	150	345	361	266	325	249	274		241 : 3	
	21	21	21	21	18	17	38	40	30	36	28	30		26 : 10	
	lines.			Rain, &c. at *Rheinburg*.											
1720	35	16	13	24	29	14	20	48	54	50	39	25	367	30 : 7	1720
1721	21	29	28	59	35	28	14	47	27	51	32	46	417	34 : 9	1721
1722	3	20	27	29	28	27	60	64	26	10	21	61	376	31 : 4	1722
1723	21	24	28	7	3	5	28	40	15	11	30	40	252	21 :	1723
4	80	89	96	119	95	74	122	199	122	122	122	172		117 : 8	
	20	22	24	30	24	19	30	50	30	30	30	43		29 : 5	

Year

Years	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	feet inch.		
	lines.			Water exhaled at *Rheinburg.*											
1720	10	10	10	25	44	55	59	56	50	30	10	12	371	30 : 11	1720
1721	8	9	15	54	47	60	48	42	27	20	22	10	362	30 : 2	1721
1722	5	7	20	28	54	54	65	59	34	14	13	13	366	30 : 6	1722
1723	7	14	33	36	58	57	37	39	24	15	15	12	347	28 : 11	1723
	30	40	78	143	203	226	209	196	135	79	60	47		120 : 6	
4	8	10	20	36	51	56	52	49	34	20	15	12		30 : 2	
	feet inch.			The height of the water in a well.									feet inch		
1720	6 :	6 : 3	6 : 5	6 : 7	6 : 6	8 :	8 : 11	9 :	8 : 3	6 : 11	5 : 8	5 : 5	84 :	7 :	1720
1721	5 : 6	5 : 6	4 : 6	4 : 2	5 : 7	7 :	8 : 1	7 : 9	7 : 7	6 : 3	4 : 6	4 : 4	70 :	95 : 11	1721
1722	6 :	5 : 7	5 : 11	6 : 1	6 : 10	7 : 11	8 :	7 : 10	7 : 4	7 : 9	7 : 3	4 : 6	81 :	6 : 9	1722
1723	5 : 4	4 : 11	5 : 7	6 : 10	8 : 1	9 : 3	9 : 7	9 : 7	9 : 9	9 : 8	9 : 3	8 : 2	96 :	8 :	1723
	22 : 10	22 : 3	22 : 5	23 : 8	27 :	32 : 2	34 : 7	34 : 2	32 : 11	30 : 7	26 : 8	22 : 5		27 : 8	
4	5 : 8	5 : 7	5 : 7	5 : 11	6 : 9	8 :	8 : 8	8 : 6	8 : 3	7 : 8	6 : 8	5 : 7		6 : 11	
	min.			The variation of a pocket watch.											
1722	÷ 34	÷ 9	÷ 7	+ 5	÷ 38	÷ 55	÷ 89	÷ 78	÷ 84	÷ 8	+ 151	+ 246	+ & ÷	402	1722
1723	+ 131	÷ 1	÷ 187	÷ 139	÷ 142	÷ 149	÷ 109	+ 113	+ 4	÷ 1	+ 246	+ 232	+ & ÷	727	1723
	+ 97	÷ 8	÷ 194	÷ 134	÷ 180	÷ 204	÷ 198	+ 35	÷ 80	÷ 9	+ 397	+ 478	+ & ÷	1129	
2	+ 48	÷ 4	÷ 79	÷ 67	÷ 90	÷ 102	÷ 99	+ 17	÷ 40	÷ 4	+ 199	+ 239	+ & ÷	564	
	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.			

A Dropsy *in the left* Ovarium *of a Woman, 58 Years of Age, cured by a large Incision made in the side of the* Abdomen; *by Dr.* Robert Houston. Phil. Trans. N° 381. p. 8.

MArgaret Millar in her last lying-in, at 45 years of age, having the burden violently pulled away, was so sensibly affected with a pain, which then seised her left side, between the navel and groin, that ever after she had scarce been free from it; but had it more or less for 13 years together; for two years before the Dr. had seen her, she was extremely uneasy, her belly grew very large, and a difficulty of breathing continually increased upon her; insomuch, that for the last six months, she had scarce breathed at all but with the utmost difficulty; and in all that time, having quite lost her appetite, she scarce eat so much as would nourish a sucking child; and for three months together she had been obliged to lie constantly on her back, not daring to move to one side or other.

This tumour was grown to so monstrous a bulk, that it engross'd the whole left side, from the navel to the *pubes*, and stretched the abdominal muscles to so unequal a degree, that the Dr. never observed the like in the whole course of his practice. It drew towards a point: Her back, by lying so long in that posture, was excoriated, which, with want of rest and appetite, had greatly wasted her.

The Dr. encouraged by the unexpected resolution and importunity of the patient, with an imposthume lancet, laid open about an inch; but finding nothing issue, he enlarged it two inches; and even then nothing came out but but a little thin yellowish *serum*: So he ventured to lay it open about two inches more. He was not a little surprised, after so large an aperture, to find only a glutinous substance stop up this orifice: All the difficulty was to remove it: He tried his probe, and his fingers, but all in vain; it was so slippery, that it eluded every touch, and the strongest hold he could take of it.

He took a strong fir-splinter, about the end of which he wrapped some loose lint and thrust it into the wound, and by turning and winding it, extracted upwards of two yards in length of a substance, thicker than any jelly, or rather like glue, that is fresh made and hang out to dry; its breadth was upwards of 10 inches: This was followed by nine full quarts of such matter, as is observed in steatomatous and atheromatous tumours, with several *hydatides* of various sizes, containing a yellowish *serum*, the least of them bigger than an orange, with

with ſeveral large pieces of membranes, which ſeemed to be parts of the diſtended *ovarium:* Then he ſqueeſed out all he could, and ſtitched up the wound in three places, almoſt equidiſtant. He was obliged to make uſe of *Lucatellus*'s balſam; with which he covered a pledget the whole length of the wound, and over that he laid ſeveral compreſſes, dipped in warm *French* brandy; and becauſe he judged, that the parts might have loſt their ſpring, by ſo conſiderable and ſo long a diſſention, he dipped in the ſame brandy a napkin, and applied it four fold over all the dreſſings, and with a couple of ſtrong towels, which were alſo dipped in the brandy, he ſwathed her round the body, and then gave her about four ounces of the following mixture.

℞ *Aq. Menthæ lb ſs. Aq. Cinnamoni fort. lb* 1 *ſs. Syr. Diacodii* ℥vi. *M.*

The cinnamon water was drawn off from canary and the beſt cinnamon, being the fineſt and moſt fragant he ever taſted; of this mixture he ordered her two or three ſpoonfuls four times a day.

Next morning he found the patient in a breathing ſwear; and ſhe informed him, that ſhe had not ſlept ſo much, nor found herſelf ſo well refreſhed, at any time for three months before: He carefully attended her once every day, and as conſtantly dreſſed her wound in the ſame manner as above, for about eight days together: He kept in the lower part of the wound a ſmall tent, which diſcharged ſome ſeroſities at every dreſſing for four or five days.

Her chief food was ſtrong broth, made of an old cock, in each porringer of which was put one ſpoonful of the abovementioned cinnamon water; this was repeated four times a day, and gave her new life and ſpirits.

She mended apace to the admiration of every body, and lived in perfect health from that time, *viz. Auguſt* 1701 till 1714, when ſhe died of 10 days illneſs.

That this tumour, or rather dropſy of the *ovarium*, was owing to the midwife's raſhneſs in pulling away the *placenta*, not knowing how to ſeparate it ſkillfully from the *uterus*, ſeems plain from what the patient herſelf told, and what afterwards happened.

The *placenta* adhering ſo faſt to the *uterus* required more art to bring it away than the midwife was miſtreſs of, which probably induced her to uſe violence; by which ſhe forced down the *fundus uteri*, and ſo overſtrained the ligaments and all that is appended to them; eſpecially, the *ligamentum latum* of the

left

left side and its *ovarium*, which may be reasonably supposed to have been hurt with the rest in the relaxation: Hence the elasticity of these parts was not only impaired, but the small lymphatics ruptured: So that the extravasated *lympha*, rushing out, thickened, and not being able to recirculate, dilated the injured *ovarium*, and thus increased the tumour; and the parts being already excessively distended, and no longer able to resist the new influx of fresh secretions, were likewise ruptured, and by degrees augmented to that enormous bulk.

Cyprianus, in his letter to Sir *Thomas Millington*, gives several instances of the mischiefs committed by midwives and other ignorant persons: When a *placenta* adheres, then says he, they tear all before them; by pulling rudely, they force down the *fundus uteri*; consequently, do violence to the *ovaria* and other parts: Whence follow inflammations, &c.

Forestus l. 28. *obs.* 80. says that an over forward midwife pulled out the *placenta* too hastily, and that the woman immediately fell into a swoon, and died directly.

Frederick Ruysch in his *obs. Anatom.* 97. attributes the cause of the close adhesion of the *placenta*, to a central insertion of the navel-string into it, in which case it is hard to separate: A train of dismal accidents attend those, who, not knowing how to do it, venture to pull it away by force.

Dionis and *La Motte* have made the same remark; by the latter we are told of innumerable accidents, which he had known happen to women in hard labour, by the rashness and ignorance of bold pretenders, who, without rule, precepts, practice, or any suitable instructions, venture to practice midwifry.

The abovementioned *Ruysch* has a remarkable instance of the effects of violence in hard labour; the case is curious. vide *Obs. Anat.* 63.

Manget, in his *Theatrum Anat.* tells us, that in all hard labours, where the *placenta* sticks, the ligaments suffer more or less by a rash and ignorant way of pulling: Examples of which see in *Platerus's Obs. lib.* 3. *Bartholin. Cent.* 2. *Hist.* 91. *Cent* 5. *Hist.* 19. *Marchett. Anat. cap.* 7. and others, of which *Blasius* has given us an ample list in his commentaries on *Vesslingius*.

He likewise tells us, that the *ovaria* grow to a surprising bulk and contain such a vast quantity of liquor, that they become dropsical; whereof *Skenckius* in his observations has recorded many examples, as has *Riolan* and several others.

Mun-

Munnics, in his *Bibliothec. Anat.* gives us the hiſtory of a large dropſy in the right *ovarium*.

Mortgagni has told us, that he has often obſerv'd large veſicles in the *ovaria*, and in morbid bodies, ſometimes full of purulent matter.

Sylvius. Ex lymphaticis læſis tam virilium quam muliebrium teſticulorum, &c. *hydrops particularis excitari poteſt.*

Veſlingius cap. 7. has often obſerv'd large tumours, from obſtructions in the *ovaria* of women.

Veſalius found in the right *ovarium* 9 or 10 glandules, like gooſe eggs, with matter not unlike the whites of eggs, or rather thicker.

Gul. Ballon. paradig. 6. found an impoſthume of the nature of a *ſteatoma*, near the *collum uteri*; the woman had a large tumour on her ſide for 16 years, and had only one *ovarium*, which was ſhewn as an extradiordinary thing.

At *Weſtminſter*, Dr. *Houſton* opened the body of a gentlewoman, about 60 years of age, whoſe left *ovarium*, of a large ſize, weigh'd ſix pounds, to which there adher'd ſeveral *hydatides* like pullets eggs.

Hildan mentions an hydropic tumour of the right *ovarium*, of a prodigious bulk, ſtuff'd with hair. vide *Gul. Fab. Hild. Cent.* 5. *Obſ. Horſt*, *Tom.* 2. *lib.* 4. *Obſ.* 53.

Ruyſch. Obſ. Anat. 17. has a paſſage to the following purpoſe, 'a dropſy, ſays he, of the *ovaria* is an affection or 'diſeaſe, well enough known to others; but I am afraid 'not ſo well conſidered: Authors have agreed to call it '*hydatis*, with which name I ſhall reſt ſatisfied, provided 'we agree about the ſubject of the diſtemper. I obſerve this 'moſt frequently, if not always to be the *ova*; but ſo 'changed and ſometimes ſwell'd to ſuch a bulk, that I have 'often ſeen them bigger than a child's head'.

Drelincourt has given us a very full and exact account of a dropſy of the left *ovarium* in a Lady of 35 years of age: The tumour, of an enormous bulk, continued three years. The body of the *ovarium*, with all contain'd therein, weigh'd 60 pounds. It was nothing but a number of little globules, cluſter'd together, that differ'd in their roundneſs, form, colour and conſiſtence, proceeding from little ſeminary veſicles in a cluſter: Some contain'd water exceeding clear and liquid; others a yellowiſh thin *ſarum*; others a glutinous matter; ſome were as big as pullets eggs; others bigger than one's fiſt.

Theſe few, out of the many inſtances could be produced from authors of undoubted reputation, ſuffice to prove, that the *ovaria*, as well as the *tubæ fallopianæ*, ligaments and *uterus* itſelf, are not free from dropſies, *&c.* and that they are owing to obſtructions, often occaſioned by rude and violent dealing with women in hard labours, which generally brings on a train of diſmal ſymptoms, that ſooner or later, according to the ſtrength of the patient, after a miſerable, painful and languiſhing life, end in death.

The manifeſt ſucceſs in the above-mentioned uncommon caſe, may be of uſe, and may ſerve to ſhew, that we ought not to deſpair ſo ſoon, in diſtempers that are ſeemingly moſt dangerous.

The Preparation of the Pruſſian Blue. Phil. Tranſ. N° 381. p. 15. *Tranſlated from the* Latin.

DR. *Woodward* communicated to the *Royal Society* the following preparation of the *Pruſſian* blue, as it was ſent him from *Germany*.

Take 4 ounces of crude tartar and crude nitre, dried, pulveriſe them very fine, and then mix them; afterwards let them decrepitate by applying a live coal; and you'll have 4 ounces of extemporaneous ſalt of tartar. While this ſalt is hot, pulveriſe it very fine, and add to it 4 ounces of ox's blood, well dried and reduced to a fine powder. After mixing them well together, put them into a crucible, ſo that it be $\frac{2}{3}$ full; then covering the crucible, put it into a fire, and ſurround it with live coals, that it may heat gradually, and that the matter may take fire and glow without too quick an accenſion: Let the matter be kept in this degree of heat, till the flame and accenſion remit; at length encreaſe the fire, till the matter be quite glowing hot, and little or no flame ariſe from the crucible; take the crucible off the fire, and putting the matter into a mortar, pound it a little, and having at hand 4 pounds *avoird.* of boiling hot rain-water, put the matter hot into it, and boil it for half an hour; after which ſtrain it thro' a linnen cloth, and pouring ſome more water upon the remaining black matter, put it again upon the fire, boil it and ſtrain it; and this muſt be repeated ſo often, till the matter be well edulcorated, and the water remain inſipid: Expreſs well all the liquor remaining in the linen

linen cloth and matter; and when you have collected the whole together, set it again upon the fire, and evaporate it to 4 pounds, and keep it by for farther use, marking it N° 1.

Take 1 ounce of *English* vitriol gently calcin'd to a white colour, dissolve it in 6 ounces of rain-water; filtre it thro' a paper, and mark it N° 2.

Lastly, take 8 ounces of crude allum; dissolve it in 4 pounds of boiling hot water, till all the alum be consum'd, after which add the solution of vitriol N° 2, which put hot from the fire into a pretty large and wide pot, and combine it with the *lixivium* N° 1, well heated apart: And immediately a great ebullition will arise, and yield the colour of borax; during the ebullition, pour it alternately out of one vessel into another; and when it ceases, let it stand; then strain it thro' a linen cloth, that the liquor may pass thro', and the colour remain in the cloth; and when no more liquor drops from it, remove it from the cloth with a wooden spatula into another smaller pot, after which pour on it 2 or 3 ounces of spirit of common salt, and there will immediately appear a very beautiful blue colour; mix this very well, and let it stand all night; afterwards put to it a large quantity of rain water, stir it about with a spatula, and when the matter has subsided, decant the water, and pour on fresh, still repeating this, till it is adulcorated, and the water that runs at length from it be insipid: Put this deep coloured blue precipitate upon a linen cloth spread, that the water may run off, and let the colour be gradually dried with a gentle heat for use.

N. B. The calcination is of considerable service in this preparation: For, a bright azure and a deep blue arise from a gentle, moderate, or strong calcination of the dried blood and salt of tartar; and hence arises the diversity of colour: The boiling hot *lixivia* are to be directly mix'd together.

Observations and Experiments on the foregoing Method of Preparation; by Mr. John Brown. Phil. Trans. N° 381 p. 17.

DR. *Woodward* having in the preceeding *Transaction* communicated to the *Royal Society* a process for making the *Prussian* blue; Mr. *Brown* was willing to go thro' it exactly, according to the proportions there prescrib'd; and he observ'd, that by a calcination of 4 ounces of blood dried with 4 ounces of the salt of tartar, in two hours that part of the operation was over, and a black spongy substance remain'd

remain'd in the crucible, weighing 4 ounces *avoirdupois*: A ſolution of which being made in boiling water, and afterwards filtred, the remainder, when dried, weigh'd 9 drachms *averdupois*.

The loſs in the ſolution and filtration of the vitriol and alum, is not worth taking notice of, they having both been very clean, before they were diſſolv'd: The mixtures being made, as preſcrib'd, with the addition of the ſpirit of ſalt, the reſult was a very fine blue, which when well edulcorated by frequent waſhings, and after that thoroughly dried, weigh'd 1 ounce or a little more, and entirely anſwer'd the character the author gave of it.

Among the ſeveral experiments, made with theſe liquors, to wit, the *livivium* with blood, the ſolution of vitriol, the ſolution of alum, and the ſpirit of ſalt; tho' they always produced a blue, yet that blue differ'd in degrees of colour, according to the varied proportions of the vitriol and alum; and the colours produced from theſe ſeveral proportions were each of them improv'd by the addition of the ſpirit of ſalt.

Mr. *Brown* only mentions two out of the ſeveral experiments he tried, in one of which the alum was entirely left out, and a pale blue produced; in the other the proportions of vitriol and alum were equal, and a very deep blue was produced.

Theſe differences in colour, ariſing from the ſeveral proportions of the vitriol and alum, are only mentioned to confirm the truth of the author's preſcript, as being the moſt exact and beſt proportioned to produce the fineſt colour, of any he tried. The only misfortune he takes notice of, attending his preſcript, is what may happen in the calcination.

It would be curious to know what gave the firſt hint for the production of ſo fine a colour, from a combination of ſuch materials; eſpecially, when we come to conſider, that the blood has the greateſt and principal ſhare in this ſurpriſing change: Mr. *Brown* doubts not, but blood, or fleſh of any kind, would produce the ſame effects, but he had reaſon to think the latter would not produce ſo beautiful a colour as the former. He on purpoſe dried ſome beef, freed from its ſkin and ſalt, and purſu'd the ſame proceſs, as with the blood; but there was a ſenſible difference obſervable during the calcination, and a very manifeſt one in the beauty of the two colours.

To.

To prove the ſhare the blood has in this change, the following experiments (ſome of which Mr. *Brown* made before e *Royal Society*) may be concluſive.

The ſolution of alum, mix'd with that of the vitriol, oduced no alteration of colour; if to theſe you add the irit of ſalt, the appearance is the ſame: But if to the whole ou put the *lixivium* with blood, there precipitates a blue.

If you ſubſtitute, inſtead of the *lixivium* with blood, a *ixivium* made with the ſame ſalt of tartar only, which then ecomes an oil of tartar; and after the mixture of the ſolu- on of alum, with that of the vitriol, you pour on this oil of rtar, there follows, it is true, a precipitation, but which is no colour; and if you add the ſpirit of ſalt, it ſo ſtrongly tracts what is precipitated, as to render the muddy mixture erfectly clear.

The very ſame effect will follow, if any volatile alkalious pirit be made uſe of as a precipitant; or any volatile ſalts iſſolv'd in water; nor can the blood itſelf be ſuppos'd to ommunicate this change from any ſuch properties, the heat f fire it undergoes in the calcination being ſufficient to throw hem off.

In the calcination of the dried blood and ſalt of tartar, it as obſerv'd, that there was a loſs of juſt the half. It is ifficult to determine exactly what quantity of either was loſt y this calcination; but it will eaſily be granted, that there as loſt a far larger quantity of the blood, than of the ſalt of rtar; and that is obvious from an experiment, by which, hen the ſalt of tartar was calcin'd by itſelf, with the ſame egree of heat, it loſt leſs than an $\frac{1}{8}$ part; whereas, when the ried blood was calcin'd by itſelf, it loſt more than $\frac{6}{8}$.

The blood, in calcination with the ſalt of tartar, communi- ates its tinging quality to the ſalt, or that quality is extracted rom it by the ſalt, and paſſes with it in its ſolution in the oiling water.

To prove this, ſome dried blood was calcin'd by itſelf, and ſtrong decoction was made of it in water, and afterwards iltred: This, when mix'd with the former ſolutions, pro- luced little or no alteration; but on the addition of the pirits of ſalt, changed to an amber colour, without any recipitation.

When this liquor was mix'd with the oil of tartar, and our'd on the former ſolutions, it caus'd a precipitation; but o colour, and the ſpirit of ſalt, as in the other experi-

ment,

ment, made the liquor clear again, but left this also of amber colour.

The change of colour is not effected in any of the materia excepting in that of the solution of vitriol; so that the alu seems only to be of use in fixing the colour, as it is often us by the dyers for that purpose, and the spirit of salt gives it deeper dye: For, if the *lixivium* with blood be poured to the solution of alum alone, there will fall a sediment, som what on the purple; to which if you add the spirit of sal it changes the colour, and the sediment becomes brown.

So, much the same changes will be produced, if you pou the spirit of salt to the *lixivium*; but not the least appearanc of a blue: Whereas, as is mentioned above, when the *lix ivium* is pour'd to the solution of vitriol, there immediatel follows the blue, which is still heightened by the addition o the spirit of salt.

It will not be improper to observe, that as the autho orders all the liquors, excepting the spirit of salt, to be boilin hot, when mix'd; so it is certain, the colour is thereby mor immediately produced, and looks more beautiful: But mos of the experiments here mentioned were made with the liquors cold, and the colours came to their beauty with a little washing. In one of the experiments with the liquors cold after the *lixivium* with blood had precipitated the blue in the mixture of alum and vitriol, by pouring in a little more of the *lixivium*, the blue all disappear'd, and a dirty muddy colour was left: But the addition of the spirit of salt soon discharged that, and the blue return'd.

In calcining the beef and salt of tartar, Mr. *Brown* found the matter left in the crucible, to weigh just half the whole mixture, as in that with the blood: But after the boiling it in water, the remainder in the filtre, when dried, was very near a third less in proportion than the other. Whence it may be reasonably inferr'd, that the salt of tartar holds a larger share of the beef in the one operation, than of the blood in the other.

Having in the former part of this account of the *Prussian* blue prov'd, by the experiments there mentioned, that the solution of vitriol was the only subject among those ingredients, that the *lixivium* of blood produced this change of colour in; and afterwards considering, that the vitriol, made use of in this preparation, is no other than iron, dissolv'd by a liquor, running from the *pyrites*, when expos'd to the weather

eather, and which is afterwards boil'd up and ſhoots into ryſtals: It ſeem'd naturally to follow, that this metal is the ubject, on which the *lixivium* of blood produces the change; nd this thought gave occaſion to the following experiments n metalline bodies, in order to obſerve, whether the ſame hange of colour could be produced in any of them.

To a ſolution of ſilver in *aqua fortis* was pour'd the *lixivium* f blood, which caus'd a *coagulum* of a pure fleſh colour. The *lixivium*, made with fleſh, produced a whitiſh *coagu-um*, and the oil of tartar (which was continued to be us'd y way of compariſon with the other *lixivia*) a much whiter *oagulum*. By the addition of the ſpirit of ſalt to each of theſe, he bloom of the fleſh colour was taken off in the firſt, but t ſuffer'd no other change. In the ſecond, the *coagulum* was omewhat tinged blue; and in the third the white was mani-eſtly improv'd. The blueiſh tinge in the ſecond of theſe xperiments cannot entirely be aſſign'd, as the effect of the *ixivium* with fleſh; becauſe ſilver, when thus diſſolv'd, whether precipitated with ſalt water, or oil of tartar, will, fter it has ſtood ſometime, contract a blueiſh tinge, and his from an alloy of copper, from which it is not entirely reed.

The ſame liquors were made uſe of to precipitate the mer-ury in the *Mercurius ſublimat. corroſ.* diſſolv'd in water; he conſequence was, that the *lixivium* with blood produced pure yellow; the *lixivium* with fleſh an orange colour; nd the oil of tartar a dingy red: The addition of the ſpirit of ſalt to theſe made ſome very odd alterations; for, the firſt changed its yellow colour for an orange; the ſecond its orange for a blue; and the third became quite clear again, without any colour. The blue colour, in the mixture of the *ixivium* with fleſh and ſolution of the ſublimate, may be accounted for from the vitriol in the compoſition of the ſub-limate: But it will not be ſo eaſy to give a reaſon, why the ſame colour ſhould not have been produced from the *lixivium* with blood, and the ſame ſolution.

Copper, diſſolv'd in *aqua fortis*, tinges the water of a green colour, and if to this you pour the two *lixivia* of blood and fleſh, the *coagula* are much alike, *viz.* a white tinged with green: But when you add the ſpirit of ſalt, they both change and become of a colour, not unlike the copper itſelf, before it is diſſolv'd in *aqua fortis*. If the oil of tartar

tartar be poured on a ſolution of the copper, the *coagulum* is a pale green, which *coagulum* the ſpirit of ſalt diſſolves and leaves the liquor clear, but green, as before precipitation.

Tin-glaſs (an imperfect metal) diſſolv'd in *aqua fortis* and mix'd with the *lixivium* of blood, produced a milky *coagulum*, and by the addition of the ſpirit of ſalt, after ſtanding ſometime, its upper ſurface changed to a light blue; both the *lixivium* of fleſh and the oil of tartar produced white *coagula*, which the ſpirit of ſalt ſcarcely alters.

Lead diſſolv'd in ſpirit of vinegar produces much the ſame white *coagulum*, when mix'd either with the *lixivium* of blood, fleſh, or the oil of tartar; nor does the ſpirit of ſalt make any alteration.

By all theſe experiments it is pretty evident, that none of theſe metalline bodies were affected by the *lixivium* of blood ſo as to produce this fine blue. The two metals untried are gold and tin; the latter of which, when diſſolv'd in ſpirit of vinegar, has ſo near a reſemblance to lead, diſſolv'd in the ſame *menſtruum*, that in all probability the experiments would anſwer much alike in both. What may be expected from gold, Mr. *Brown* was not hitherto ſo well aſſur'd of, as he was from iron; which, when diſſolv'd in ſpirit of vitriol, will anſwer all the experiments that have been tried with the ſolutions of vitriol, and produce as fine a colour; nor can this be owing to any property in the diſſolvent itſelf which, tho' drawn from the ſame kind of vitriol all along made uſe of in theſe experiments; yet is ſo alter'd by the violent fire in the production of it, as not to anſwer in any trials to the vitriol itſelf.

May we not, therefore, hence conclude, that iron is the ſubject of this beautiful colour, produced by means of the *lixivium* with blood.

The Remainder of the Bills *of* Mortality, *&c. of the ſeveral Towns in* Europe, *extracted from the* Acta Breſlavienſia; *by Dr.* Sprengell. Phil. Tranſ. N° 381. p. 25.

A Liſt of thoſe chriſtened and buried in Breſlaw *from the 25th. of* December 1719. *to the 24th of* December 1720, *inclusive.*

Buried.	
From the 25th of December, to the 31ſt,	40
In January	160
February	107
March	139
April	192
May	158
June	120
July	131
Auguſt	182
September	189
October	130
November	132
December, only till the 24th	127
	1816

Chriſtened.		
Males		564
Females		556
		1120
Married 460 pair.		
Among the dead were		
Married men		385
Married women		186
Widows and widowers		285
Batchelors		113
Maidens		113
Children to ten years of age.	Boys	345
	Girls	300
Stillborn	Boys	53
	Girls	30
		1810

In the year 1720 in Lignitz were

Married	92 pair.
Chriſtened	283
Buried	366
Among which were children.	149

In Jauer.

Chriſtened of Proteſtants	100 Ch.
Buried	175

In Oels.

Chriſtened	180
Buried	203

In Schweidnitz.

Married	218 pair.
Chriſtened	920
Buried	250

In Vienna.

From the 1ſt of *January* to the 31ſt of *December.*

Buried	6825
Chriſtened	4126

In Lobau.

Married	58 pair.
Born	160
Buried	355

Amongſt which were born 80 boys and 80 girls: Buried 95 men and 48 women, beſides 4 who died in childbed: 26 batchelors and 22 maidens: 36 boys and 42 girls: 25 chriſoms: 7 ſtillborn: 55 widows and Widowers. Likewiſe 51 perſons between 60 and 70 years of age: 29 between 70 and 80: 7 between 80 and 90: 1 of 90, and 1 of 99.

In Dreſden.

Married 368 pair.
Born 1448
Among which were 719 boys, and 641 girls, legitimate: Illegitimate boys 44, and girls 44.
Buried 1733
Amongſt which were 255 married men, and 182 married women, 189 widowers and widows: 88 young men and 84 young or ſingle women: 883 children, *viz.* 461 boys and 422 girls, amongſt which were ſtillborn 72, *viz.* 37 boys, and 35 girls.

In John-George *city*.

Married 24 pair.
Chriſtened 148
i. e. 79 boys and 69 girls, among which were 2 ſtillborn, and 10 baſtards.
Buried 243
viz. 37 married men and 18 married women: 5 widowers and 37 widows, 6 batchelors, and 18 maidens: 64 boys, 58 girls, *&c.*

In St. Annaberg.

Married 18 pair.
Born 105
i. e. 63 boys and 42 girls, among which were 2 pair of twins, and 4 baſtards.
Buried 180
viz. 96 men and 84 women; among which were 34 married men and 18 married women, 1 in childbed; 12 widowers and 28 widows: 12 batchelors, and 17 maidens: 38 boys and 20 girls among which was 1 dead born.

In Schneeberg.

Married 12 pair.
Born 89
As 44 boys: 45 girls; among which were 4 ſtill-born, and 3 baſtards.
Buried 157
viz. 28 married men, and 11 married women: 1 childbed woman: 5 widowers and 25 widows: 9 batchelors and 10 maidens: 68 children, *viz.* 49 boys and 19 girls.

In Leipſic.

Married 314 pair.
Born 790 *viz.* 405 boys and 385 girls.
Buried 1264
Among which were 233 men and 146 women: 86 batchelors, and 62 maidens: 23[illegible] boys and 195 girls: nine women

women in childbed: 98 chriſoms, *i. e.* 53 boys and 45 girls: 58 ſtill-born, as 36 boys and 22 girls.

In Berlin.

Married	669 pair.
Born	2279
Buried	2426

In Wiſmar.

Chriſtened	168
Buried	100

In Epperies.

Born	171
Buried	116

In Rawitz.

Born 134

And juſt as many males as females, except two pair of twins.

Buried 95

Among which were 15 childbed women, and 3 ſtill-born.

In Dantzick.

Born	1862
Buried	1610
Married	442

This is merely of the city, and not of the ſuburbs of *Dantzick*.

A Liſt of the ſeveral Cities and Towns in the Kingdom of Pruſſia.

In the Year 1720.	*Chriſtened.*	*Pair marr.*	*Buried.*
In Angerberg	535	88	259
Balga	638	113	395
Bartenſtein	224	63	188
Barthen	337	66	246
Brandenburg	936	213	576
Teutſch-Eulau	100	25	82
Fiſhauſen	478	103	270
Gerdauen	489	98	206
Gilgenburg	225	56	137
Inſterburg	2386	336	1398
Johannisburg	378	68	188
Labiau	616	92	273
Liebſtadt	440	73	343
Lotzen	256	45	148
Luck	364	65	174
Marienwerder	769	145	641
Mummel	953	168	448
Neydenburg	512	104	437
Neuhauſen	243	62	141

	Chri-stened.	*Pair marr.*	*Buried*
In Neuhoff	28	6	10
Oletzko	688	111	261
Ortelsburg	476	82	330
Osterode	784	92	300
Preuss Eulau	479	112	282
——— Marck	705	130	478
——— Holland	705	125	557
Ragnit	930	93	516
Rastenburg	610	155	452
Rhein	482	89	193
Schacken	576	96	259
Schonberg	276	51	230
Sehesten	354	62	181
Taxium	876	157	441
Tilsit	1139	187	647
The City of Koningsberg	1682	474	1402

In the year 1721. In *Breslaw*

Buried.	
From the 25th of December to the 31st 1720.	38
January, 1721.	123
February	129
March	161
April	127
May	149
June	132
July	86
August	103
September	125
October	99
November	121
December till the 24th,	89
Total	1482

Christened.		
Males		610
Females		585
Total		1195
Married		405 pair
Among the dead were.		
Married men		301
——— Women		157
Widows and widowers		208
Batchelors		92
Maidens		82
Children to ten years of age	Boys	324
	Girls	248
Stillborn	Boys	42
	Girls	28
Total		1482

In Jauer.

Buried 114 Chriſtened 124

In Vienna.

Buried 6490 Chriſtened 4104

So that there died 2386 more than were chriſtened.

Among the dead were 43 caſualties.

Beſides 8 perſons of 90 years of age

1	91
2	92
3	93
3	94
3	96
1	98
4	99
2	100
1	105

In Dreſden.

Buried 1850 Chriſtened 1396 Married 404 pair.

Amongſt the buried were.	
Married men	274
——— women	206
Widowers	42
Widows	238
Batchelors	128
Maidens	93
Boys	479

Girls		400
Still born	Males	50
	Females	28
Among the baptiz'd were,		
Boys		701
Girls		690
Baſtards	Males	40
	Females	51

In LEIPZICK

Buried.

	Married men	Married women.	Batchelors.	Maidens.	Boys.	Girls.	Childbed women	Chrysoms. Males.	Chrysoms. Females	Stillborn. Males.	Stillborn. Females.	Widowers & wid.	Summa.
January	28	25	13	11	15	14	1	1	1	2	3	20	134
February	17	12	15	9	16	10	2	4	3	7	2	17	114
March	28	29	13	12	14	15	0	1	3	3	3	17	138
April	31	24	9	10	17	22	2	2	3	2	0	13	135
May	25	23	10	9	23	16	3	6	5	0	0	16	136
June	28	16	17	4	15	13	2	3	6	2	2	12	120
July	7	10	7	5	8	12	2	1	4	0	3	15	74
August	14	8	6	6	19	11	3	2	3	3	2	9	86
Septem.	14	14	1	2	13	18	2	4	4	1	1	7	81
October	17	10	4	7	14	20	6	7	2	7	4	5	103
Novemb.	12	13	3	6	20	7	1	3	7	2	0	11	85
December	16	12	2	12	19	17	10	3	3	2	1	7	94
Summ.	237	169	100	93	193	175	24	37	44	31	21	149	1300

Among the dead were
218 from 60 to 69 years
82 70 79 old.
16 80 88
2 90 91

Besides 1 batchelor of 60, and 2 of 61; and 5 maidens of 60, 62, 66, 70, and 73 years of age.

Christened

Chriſtened.

	Males.	Fe-males.	Total.	Pair marr.
January.	43	31	74	13
February.	31	23	54	30
March.	34	31	65	1
April.	32	38	70	
May.	31	22	53	20
June.	21	31	52	24
July.	37	26	63	21
Auguſt.	27	38	65	17
Septem.	29	46	75	30
October.	38	33	71	20
Novem.	37	34	71	52
December	27	20	47	1
Sum.	387	373	760	268

Among the chriſtened were. Eight poſthumous; 14 twins; 63 baſtards; and 2 *Jews* baptized.

So that in this year there died 1300. Born and chriſtened 760. Hence there are 540 fewer born than died.

In the *Marck*.

Chriſtened 16086 Amongſt which were 596 baſtards.
Married 4613 pair.
Buried 13511

More born 2575 than buried.

Among the buried were 28 perſons of 90 years of age and upwards, beſides 3 of 100, 1 of 101, 1 of 102, 2 of 104, 2 of 107, and 1 of 112 years.

In the whole *Royal Pruſſia*.

Born 75275. Buried 58017. That there are 17258 more born than buried.

Next

Next follows the Special List of the Kingdom of Prussia.

	Christened.	*Pair Marr.*	*Buried.*
In Angerberg	518	104	282
Balga	605	168	381
Bartenstein	278	65	218
Bahrten	357	64	208
Brandenburg	944	222	695
Dutch-Elau	103	20	78
Fishausen	498	112	300
Gerdauen	490	75	219
Gilgenburg	110	37	76
Insterburg	2235	359	889
Johannisburg	463	76	174
Labiau	569	92	277
Liebstadt	431	94	252
Lotzen	247	68	180
Lyck	352	96	183
Marienwerder	765	162	400
Mummel	914	193	522
Neydenburg	545	121	326
Neuhausen	234	59	138
Neuhoff	34	10	40
Oletzko	631	103	312
Ortelsburg	497	92	282
Osterode	369	144	183
Preuss Eylau	503	113	428
—— Holland	605	166	376
—— Marck	657	134	335
Rangnit	791	95	336
Rastenburg	738	165	537
Rhein	416	108	211
Shacken	514	87	326
Schonberg	255	61	147
Sehesten	330	80	233
Taxium	870	159	508
Tilsit	1245	185	584
the City of Koningsberg	1655	424	1776
Total	20768	4313	12406

In

In *Ratisbon*

Christened Males --- 130
Females -- 120

Total 250

Amongst which were 3 pair of twins, *viz.* 2 boys and 4 girls. Besides 6 bastards, *i. e.* 3 boys and 3 girls.

Buried 220. *viz.* Married men 41. Married women 43. Young men 11. Young women 15. Children 110. *i. e.* 57 boys and 43 girls. Amongst which, widows 23. Lying-inn women 2. Stilborn 2. Married 67 pair.

In *Nurnburg.*

Buried		Christened.	
Married Men	234	Males	541
—— Women	257	Females	543
Bachelors	48		
Maidens	73		Total 1084
Boys	232		
Girls	191		
Stilborn	18		
In the Suburbs	10	21 more born than died, amongst the christened were 16 pair of twins.	
Total	1063		

In *Copenhagen.*

Born 2630 Buried 2247.

In *Amsterdam.*

The following list contains 7 years, *viz.* from 1715 to 1721.

Anno	1715	died 7633 Persons
	16	7078
	17	7451
	18	8644
	19	9726
	20	7820
	21	7632

In *Epperies.*

Born 214. Buried 142.

In *Dantzick.*

Born 1833. Buried 1435. Married 457 Pair.

An Account of the Diſſection of an Eye *with a* Cataract; *by Mr.* John Ranby. Phil. Tranſ. N° 381. p. 36.

SEPT. 21, 1723. Mr. *Ranby* was conſulted by one *William Sollars*, 50 years of age, who complained of a decay in his ſight: Upon examining his eyes, he obſerved two cataracts, that in his right eye almoſt ripe, the other juſt forming. There being no other obvious method of relieving the patient, Mr. *Ranby* propoſed the operation; but firſt adviſed him to conſult ſome others of the profeſſion; and accordingly adviſing with Mr. *Tanner*, he, upon viewing his eyes, declared there both cataracts, but that neither of them was yet fit for the operation. In the mean time the patient fell ill of a fever, and died the ſecond of *March*. Mr. *Ranby* procured the right eye, in which the cataract was moſt confirmed, in order to make an exact diſſection of it: In examining it, he found the aqueous and vitreous humours in their natural ſtate; but the cryſtalline humour opaque and of a foul pearl colour, and more ſolid ſubſtance than uſual. The generally received opinion, that a cataract is a film, growing behind or ſometimes before the pupil, made Mr. *Ranby* examine both chambers of the aqueous humour, with the utmoſt exactneſs, but all to no purpoſe; for the aqueous humour had its natural tranſparency, nor could he obſerve any thing preternatural, either on the *iris* or *uvea*, excepting too great a contraction of the pupil. This very much confirms the opinion of *Maitrejean*, *Briſſé*, *Heiſter* and *Valſalva*, who have ſeverally aſſerted, that a cataract is only an opacity of the cryſtalline humour; and that it naturally proceeds from a ſerous acid, which ſo far aſtringes, and corrodes its ſubſtance, as to deſtroy its tranſparency. This *Maitrejean* confirms by an experiment of immerging the cryſtalline humour in a compoſition of three parts water and one of *aqua fortis*, by which he tells us it may be rendered hard and opaque: But in this point Mr. *Ranby* is of the ſame opinion with the learned Dr. *Pitcairn*, who has ſufficiently proved, that there is no ſuch ſerous acidity in an animal body. To Mr. *Ranby* nothing ſeems more eaſy than to deduce this opacity of the cryſtalline humour from an inflammation in the blood, or an increaſed *momentum* in the fluids, with which it is ſupplied: For, in that caſe groſſer particles, inconſiſtent with the tranſparency, may be impelled into the lymphatic veſſels, of which it is compoſed; and that there is an inflammation is ſufficiently demonſtrable hence, The patient often feels a pungent pain in the eye, which, as

it

t is generally the forerunner of a cataract; ſo it certainly indi-cates an inflammation of the part. 2. Theſe *maculæ*, which appear, ſwimming in the air, as it were, plainly prove, that here are opaque particles already entered into the lymphatic veſſels, which compoſe the vitreous humour. 3. The *iris*, whoſe colour ariſes from the blood-veſſels, as it changes from a lighter to a darker colour, ſhews the violence of the inflamma-tion; and is, therefore, eſteemed a ſymptom of the worſt con-ſequence.

Obſervations on the Comet, *that appeared in* October, No-vember *and* December 1723; *by Mr.* Bradley. Phil. Tranſ. N° 282. p. 41.

THE ſmall comet, ſeen in theſe parts of *Europe* in *No-vember*, *October* and *December* 1723, was firſt obſerved n *England* by Dr. *Halley October* 9, between ſeven and eight o'clock in the evening; it appearing then to the naked eye not much unlike a ſtar of the third magnitude: Viewing it thro' a teleſcope, he ſaw ſome ſmall teleſcopic ſtars near it, whoſe ſitua-tion he noted together with that of the comet, in order to ſee which way it tended. About 9 o'clock he again viewed the comet, and found it conſiderably moved from its former ſtation, having now paſſed a ſmall ſtar, which at the time of the firſt obſervation was on the other ſide of it: Comparing the two ſituations of the comet together, he perceived, that its appa-rent motion at that time was about 8 or 9 minutes in an hour, in a direction towards *ſagitta*; and that the comet paſſed very near it, if not entirely eclipſe the abovementioned ſmall ſtar, whoſe place he afterwards found to be in 7° 22′ 15″ of *Aqua-rius*, with 5° 2′ N. Lat. From the ſituation of the comet at the time of the firſt obſervation, he judged it was in conjunction with the ſtar at 8^h 5′ equal time.

Note, that the equal (and not the apparent) time, is like-wiſe made uſe of in all the following obſervations.

October 10. Dr. *Halley* having communicated to Mr. *Bradley* the ſubſtance of what he had obſerved, the latter was enabled the night following to ſee the comet at *Wanſted*: The clouds hindered him from obſerving it in the manner he deſigned; but he had time enough to meaſure its diſtance (with a micrometer in a 7 foot teleſcope) from a ſtar in *Aquarius*, marked ε by *Bayer*. At 6^h 21′ the obſerved diſtance between this ſtar and the comet was 1° 15′ 53″, and a great circle paſſing thro' the ſtar and comet made an angle with the vertical circle of 60° and

¼. The comet was more ſoutherly and weſterly than the ſtar. By this obſervation the comet preceeded the ſtar 1° 3′ 50″ *R. Aſcen.* being 39′ 5″ more ſoutherly: So that the comet's *R. Aſcen.* was 307° 6′ 40″ and its declination 11° 8′ 15″ S.

The place of ε here aſſumed is according to the *Catal. Brit.* as are alſo the places of the other ſtars hereafter mentioned, from which the comet was obſerved. The right aſcenſions and declinations, here ſet down, of the ſeveral ſmall ſtars, that are not in that catalogue, were determined by obſerving the differences of *R. Aſcen.* and declination between thoſe ſmall ſtars and others that were in the catalogue, and had nearly the ſame declinations.

The ſame evening at 7^h 3′ a ſmall ſtar that was more eaſterly than the comet, and had much the ſame declination with it, was diſtant from it 35′ 40″. About the ſame time another ſmall ſtar, that had nearly the ſame *R. Aſcenſ.* with the comet, but was more ſoutherly, was diſtant from it 39′ 58″. The places of theſe two ſtars he had not hitherto obſerved.

The next night proved cloudy; ſo that Mr. *Bradley* could not ſee the comet again till *October* 12. when, the air being very ſerene and clear, he (Mr. *Pound* aſſiſting in this and the following nights obſervations) had an opportunity of comparing it with two or three ſmall ſtars, that were near it.

At 7^h 22′ a ſmall ſtar, whoſe right aſcenſion was found 304° 40′ 23″, and its declination 7° 8′ 22″ S. preceeded the comet in *R. Aſcen.* 26′ 21″ being 10′ 42″ more northerly: Hence the comet's *R. Aſcen.* was 305° 6′ 44″ and its declination 7° 19′ 4″ S.

At 8^h 50′ the comet was in the ſame parallel of declination with another ſmall ſtar, whoſe *R. Aſcen.* was found 305° 9′ 56″ and its declination 7° 13′ 20″ S. and preceeded the ſaid ſtar 6′ 20″ *R. Aſcen.* Hence the *R. Aſcen.* of the comet was 305° 3′ 36″ and its declination 7° 13′ 20″ S. Theſe obſervations were made with a 15 foot teleſcope, furniſhed with a micrometer; as were alſo all thoſe of the following nights.

The next night, *viz. October* 13, 6^h 58′ the comet followed a ſmall ſtar 4′ 10″ *R. Aſcen.* being more northerly than the ſtar 11′ 45″. The clouds did not permit to obſerve the place of this ſtar; but its right *Aſcen.* muſt be about 304° 22′ and its declination 6° 10′ S.

October 14. The comet was near 2 ſtars, which are the 66th and 67th of *Aquila* and *Antinous* in the *Catal. Brit.* and at 8^h 57′ it followed the ſouthermoſt of them 20′ 37″ *R. Aſcen.* being

being 29′ 8″ more ſoutherly: Hence the comet's *R. Aſcen.* was 303° 49′ 10″ and its declination 4° 43′ 54″ S.

October 15. 6ʰ 35′ the comet preceeded the northermoſt of the ſaid ſtars 23′ 6″ *R. Aſcen.* being more ſoutherly than the ſtar 4′ 15: Hence the *R. Aſcen.* of the comet was 303° 24′ 40″; its declination 3° 51′ 3″ S.

October 21. 6ʰ 22′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 7′ 17″ and its declination 11′ 50″ S.) preceeded the comet 41′ 6″ *R. Aſcen.* being 5′ 50″ more ſoutherly. Hence the comet's *R. Aſcen.* was 301° 48′ 23″ and its declination 6′ S.

October 22. 6ʰ 24′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 39′ 47″ and declination 32′ 43″ N.) followed the comet ½ a minute *R. Aſcen.* being 13′ 43″ more northerly: Hence the comet's *R. Aſcen.* was 301° 39′ 17″ and its declination 19′ N.

October 24. 8ʰ 2′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 24′ 57″ and its declination 1° 9′ 22″ N.) preceeded the comet 37″ *R. Aſcen.* being 5′ 12″ more northerly. Hence the comet's *R. Aſcen.* was 301° 25′ 34″ and its declination 1° 4′ 10″ N.

October 29. 8ʰ 56′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 6′ 20″ and declination 2° 51′ N.) preceeded the comet 1′ *R. Aſcen.* being 23′ 40″ more northerly: Hence the comet's *R. Aſcen.* was 301° 6′ 20″ and its declination 2° 27′ 20″ N.

October 30. 6ʰ 20′. The ſame ſtar had exactly the ſame *R. Aſcen.* with the comet, being 11′ 33″ more northerly: Hence the comet's *R. Aſcen.* was 301° 6′ 20″ and its declination 2° 39′ 27″ N.

November 5. 5ʰ 53′. A ſmall ſtar (whoſe *R. Aſcen.* was found 300° 35′ and its declination 3° 45′ 30″ N.) preceeded the comet 33′ in *R. Aſcen.* being 2′ 8″ more ſoutherly: Hence the comet's *R. Aſcen.* was 301° 8′ and its declination 3° 47′ 38″ N.

November 8. 7ʰ 6′. A bright ſtar placed by *Hevelius* in *Roſtro Aquilæ*, but not inſerted in the *Catal. Brit.* (whoſe *R. Aſcen.* at this time was found 302° 21′ 30″ and declination 4° 28′ 4″ N.) followed the comet 1° 7′ 40″ in *R. Aſcen.* being 13′ 3″ more northerly: Hence the comet's *R. Aſcen.* was 301° 13′ 50″ and its declination 4° 15′ 37″ N.

November 14. 6ʰ 20′. A ſtar (whoſe *R. Aſcen.* was found 301° 27′ 10″ aud its declination 4° 59′ 40″ N.) preceeded the comet 5′ 35″ in *R. Aſcen.* being 5′ 50″ more ſoutherly: Hence the comet's *R. Aſcen.* was 301° 32′ 45″ and its declination 5° 5′ 30″ N.

This

This was the laſt time Mr. *Bradley* obſerved the place of the comet, till after the full moon.

Dr. *Halley* and Mr. *Graham* continued to obſerve the comet till *November* 20. and according to both their obſervations that evening at 7^h 45′ the comet followed β in *Collo Aquilæ* 6° 33′ 55″ in *R. Aſcen.* being about 4′ more northerly than the ſtar: Hence the comet's *R. Aſcen.* was 301° 59′ 50″ and its declination 5° 48′ 55″ N.

The light of the moon increaſing daily, prevented them from making any more obſervations, the comet being by this time grown ſo faint, as to become in a manner imperceptible, while the moon ſhine bright: And the faint appearance it made before the moon obſtructed the ſight of it, gave little hopes of its being ſeen again after the full moon. Notwithſtanding which Mr. *Bradley* (being then near *Cirenceſter* in *Glouceſterſhire*) was tempted by the ſerenity of the evening, and the uſe of a very good 10 foot teleſcope, to look for it again before the moon roſe; and he found it among ſome ſmall teleſcopic ſtars; but it appeared ſo faint and dull, as made it doubtful, whether what he took for the comet might not be a ſmall ſtar with a little hazineſs about it. But this doubt was cleared two nights after, when he perceived that the comet was moved from its former ſituation, towards a bright teleſcopic ſtar, from which he afterwards took its difference of *R. Aſcen.* and declination upon his return to *Wanſted* on *December* 7.

This ſtar's *R. Aſcen.* was then found to be 303° 39′ 20″ and declination 7° 32′ 30″ N. And *December* 7. 6^h 45′ the comet followed it 3′ 15″ in *R. Aſcen.* being 14′ more northerly than the ſtar: Hence the comet's *R. Aſcen.* was 303° 42′ 35″ and its declination 7° 46′ 30″ N.

This was the laſt night Mr. *Bradley* ſaw the comet, an uninterrupted ſucceſſion of cloudy evenings ſo long prevented his obſerving it, that it became uncertain where to look for it.

The abovementioned obſervations are the principal of thoſe made at *Wanſted*; and moſt of them being taken from ſtars, which are not in the *Britiſh* catalogue, whoſe places, therefore, are here determined, only by comparing them with ſome that were; it cannot be ſuppoſed, that the comet's places, deduced from them, are altogether exact. For which reaſon Mr. *Bradley* has all along ſet down not only the place of the comet and ſtar, where it was known; but alſo the particulars of the obſervation, that if any hereafter ſhould be willing to examine the track of this comet more nicely, they may know where to find

the

the ſtars, from which it was obſerved. The places of the ſtars here ſet down are abundantly ſufficient for that purpoſe, as will appear from the following table, which contains the longitudes and latitudes of the comet, deduced from the foregoing obſervations; together with the places of the comet, calculated from the theory of gravity, for the times of obſervation on the ſeveral days therein mentioned; as alſo the differences between the obſerved and computed places. Thoſe differences, not exceeding 1 minute, ſhew, that the obſervations are not only conſonant to each other, but that the places of the ſtars are likewiſe near the truth; ſince the comet's places, deduced from them, are found all along to agree ſufficiently near with the theory of gravity; the truth of which having long ſince been eſtabliſhed by its great author Sir *Iſaac Newton* and by Dr. *Halley*, needs not the confirmation of ſo ſhort a ſeries of obſervations, as was made of this comet: But ſhort as it is, Mr. *Bradley* preſumes, it will be no eaſy matter to account for the obſervations with the ſame degree of exactneſs any other way than by that theory, according to which the following computations are made.

1723. equat. time				Long. of comet obſ.			N. Lat. obſerved			Long. of comet comput.			N. Lat. computed			Differ. of Long.		Differ. of Lat.	
	d.	h.	′	♒ °	′	″	°	′	″	♒ °	′	″	°	′	″		″		″
Octob.	9	8	5	♒ 7	22	15	5	2	0	♒ 7	21	26	5	2	47	+	49	−	47
	10	6	21	6	41	12	7	44	13	6	41	42	7	43	18	−	30	+	55
	12	7	22	5	39	58	11	55	0	5	40	19	11	54	55	−	21	+	5
	14	8	57	4	59	49	14	43	50	5	0	37	14	44	1	−	48	−	11
	15	6	35	4	47	41	15	40	51	4	47	45	15	40	55	−	4	−	4
	21	6	22	4	2	32	19	41	49	4	2	21	19	42	3	+	11	−	14
	22	6	24	3	59	2	20	8	12	3	59	10	20	8	17	−	8	−	5
	24	8	2	3	55	29	20	55	18	3	55	11	20	55	9	+	18	+	9
	29	8	56	3	56	17	22	20	27	3	56	42	22	20	10	−	25	+	17
	30	6	20	3	58	9	22	32	28	3	58	17	22	32	12	−	8	+	16
Nov.	5	5	53	4	16	30	23	38	33	4	16	23	23	38	7	+	7	+	26
	8	7	6	4	29	36	24	4	30	4	29	54	24	4	40	−	18	−	10
	14	6	20	5	2	16	24	48	46	5	2	51	24	48	16	−	35	+	30
	20	7	45	5	42	20	25	24	45	5	43	13	25	25	17	−	53	−	32
Dec.	7	6	45	♒ 8	4	13	26	54	18	♒ 8	3	55	26	53	42	+	18	+	36

In order to determine the orbit of this comet, Mr. *Bradley* ſuppoſed it to deſcribe a parabola, agreeable to what is delivered in *lib.* 3. of Sir *Iſaac Newton's Princip. Math.* and then he found the inclination of the planes of the orbit and ecliptic 49° 59′; the place of the aſcending node 14° 16′ of *Aries*; the place of the perihelion 12° 52′ 20″ of *Taurus*; the diſtance of the perihelion from the node 28° 36′ 20″; the logarithm of the perihelion diſtance 9.999414; the logarithm of the diurnal motion 9.961007; the time of the comet's being in its perihelion *September* 16. 16^{h} 10′ equal time. The motion of the comet in its orbit, thus ſituated, was retrograde or contrary to the order of the ſigns.

From theſe elements, by the help of Dr. *Halley's* general table for comets, (to which they are adapted) Mr. *Bradley* computed the places in the foregoing table, which agreeing with the obſerved places as near as the obſervations themſelves agree with each other, ſhew, that it would be a vain attempt to pretend to determine the true ellipſis in which this comet moves, or its periodical revolution, from ſo ſmall a part of its orbit as it deſcribed between the firſt and laſt of the foregoing obſervations: This, therefore, muſt be left to poſterity; eſpecially, ſince it is certain, that this comet is not one of thoſe of which obſervations have hitherto been tranſmitted to us, ſufficient to determine the ſituation of their orbits.

The *nucleus* of this comet was very little; for, it appeared but of a ſmall diameter, when Mr. *Bradley* firſt ſaw it; tho' it was then above three times nearer the earth than the ſun is at its mean diſtance: Its tail was then hardly diſcernible with the naked eye; but thro' a teleſcope one might perceive a faint light, extending itſelf above a degree from the body.

Mr. *Bradley* had not hitherto heard, that this comet was ſeen before *October* 6. tho' it was in a proper ſituation to have been obſerved in the morning, the moſt part of *September*; eſpecially, from the time it was in its perihelion, till near the end of that month: For, about that time it croſſed the *milky way* between the *maſt of the ſhip* and the head of the *Great dog*, paſſing between the bright ſtars in the body and tail of the *Great dog*, towards the head of the *dove*, where it was about *September* 29. being by that time got ſo far towards the ſouth pole, as not to riſe above our horizon. From thence it paſſed under the tail of *Xiphias* within about 15° of the ſouth pole of the ecliptic; and moving on between the head of *hydrus* and the bright ſtar in *Eridanus*, called *Acharnar*, it went by the ſtars

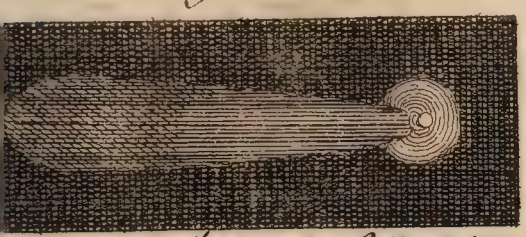
in the body and neck of the *crane* about *October* 5. when it came again above our horizon: From hence paſſing under the tail of the ſouthern *fiſh*, and between the ſtars in the ſhoulder of *Capricorn*, it croſſed the ecliptic *October* 8. in about 8° and ½ of *Aquarius*: From thence it moved on by the hands of *Aquarius* and *Antinous* towards the head of the *eagle*, according to its courſe before deſcribed.

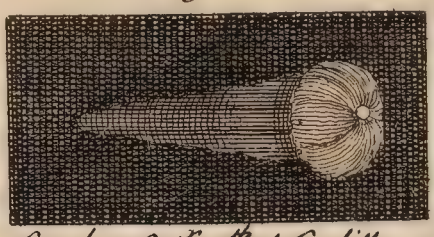
The comet was in oppoſition to the ſun *October* 1. when it had near 74° S. Lat. and altered its longitude 2 ſigns in a day. About *October* 3. it was in its *perigæum* or neareſt diſtance to the earth, being then almoſt 10 times nearer it than the ſun is at its mean diſtance; and its apparent motion was then about 20° in a day; and when Mr. *Bradley* laſt ſaw it, it was above twice as far off as the ſun.

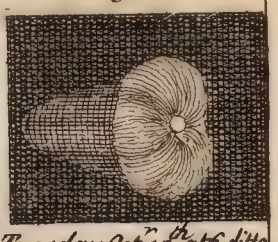
Obſervations on the ſame Comet *at* Witham *in* Eſſex; *by the Lord* Paiſley. Phil. Tranſ. N° 382. p. 50.

HIS Lordſhip being at *Witham* in *Eſſex*, where he had the advantage of a very clear ſky, firſt diſcovered this comet on *Friday* the 11th of *October*, 1723, about 7 o'clock in the evening; it then appeared not much unlike a ſtar of between the fourth and fifth magnitude; but a hazineſs round the head, and ſome light ſtreaming from it on that ſide, oppoſite to the ſun, induced him immediately to look upon it as a ſmall comet; which his obſervation next evening abundantly ſatisfied him of. He was very particular in the notice he took of its appearance, Fig. 1, 2, 3. Plate XI. repreſenting it on three ſeveral nights, *viz.* the 11th 13th and 15th of the ſame month: Some time after the tail became ſo inconſiderable, as hardly to deſerve any farther deſcription, as will be readily judged from the decreaſe of it between the 11. and 15. days of the month. The tail was viſible on the 11. to near a degree's diſtance from the body, as he found by comparing it with ſome known diſtances in the heavens; it was of a duſky light, not unlike a cloud growing darker and darker towards its extremity, as repreſented in Fig. 1. where, as well as in the two following Fig. the whole ſpeck in the head is deſigned to ſhew the brightneſs of a ſmall ſtar; from the compariſon of which with the tail, the brightneſs of the latter may in ſome ſort be collected. The tail appeared ſharper and not ſo much ſpread in the two following obſervations, and in the laſt did not exceed ⅓ part of the firſt length; it was then of a much darker colour, which made the difference between that and the head more obſervable, the head

Fig. I. Fig. II. Fig. III.







iday Oct.r 11.th at 7. in ye Evening Sunday Oct.r 23.th at 6. ditto. Tuesday Oct.r 25.th at 6 ditto

Fig. IV.

Fig. V.

Fig. VII.

Fig. VI.

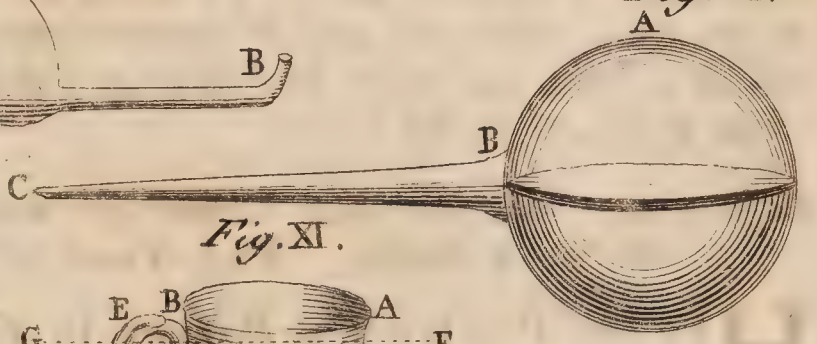


Fig. XI.

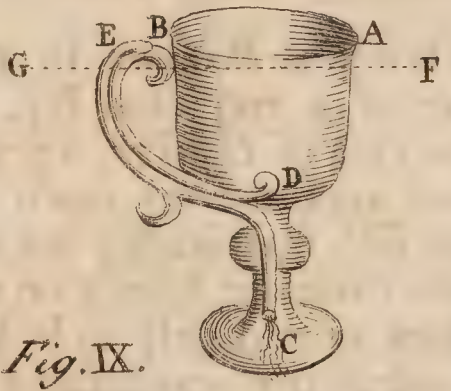
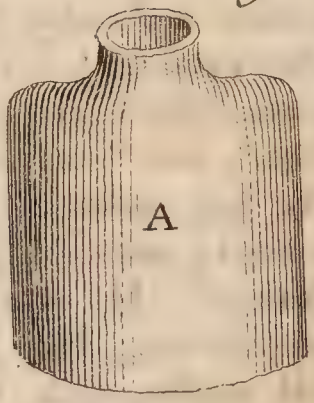


Fig. VIII



ig. X.

Fig. IX.

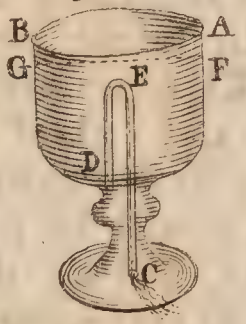


Fig. XII.

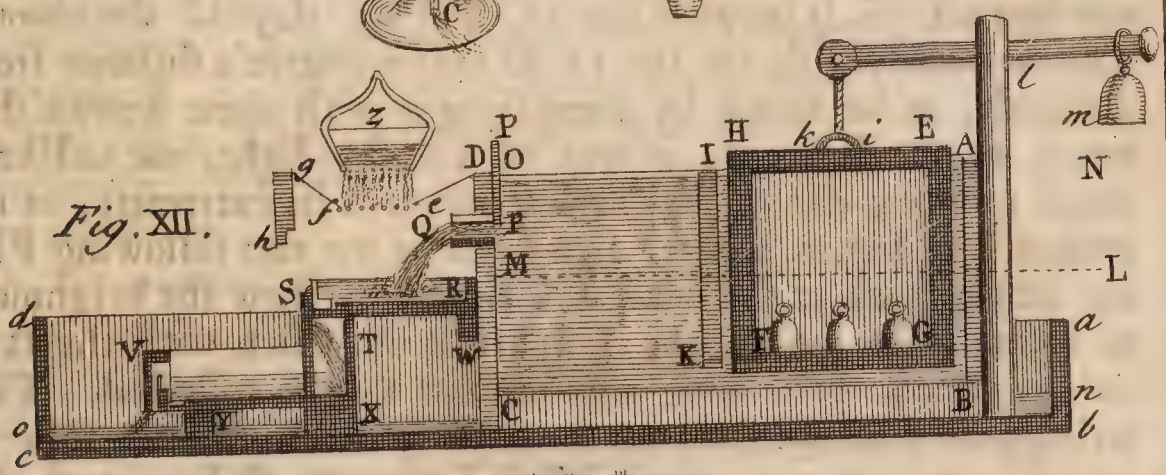
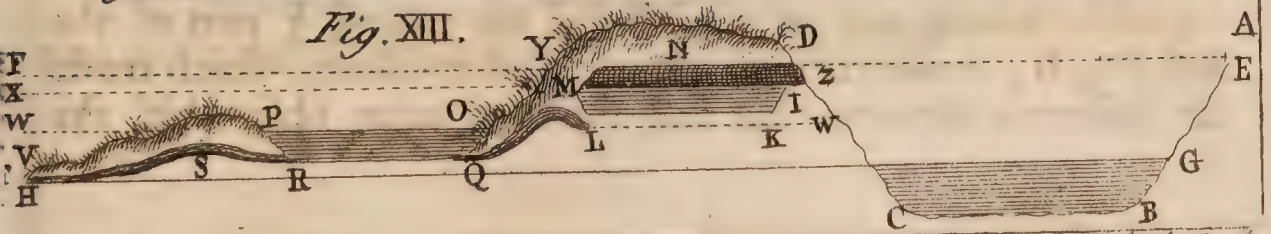


Fig. XIII.



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head ſtill appearing ſufficiently bright. For ſome following nights his obſervations were interrupted by cloudy weather, after which the comet was ſo far diminiſhed, as only to be known by its motion, its appearance being no ways diſtinguiſhable from that of a ſmall nebuloſe ſtar.

An Obſervation on the ſame Comet *at* Albano October 1723; *by* S. Franciſco Bianchini. Phil. Tranſ. N° 382. p. 51. *Tranſlated from the* Latin.

OCTOB. 17. S. *Bianchini*, after obſerving the ſituation of *Jupiter*'s ſatellites, chanced to caſt his eyes on *Capricorn*; and running over all the ſtars in that conſtellation, he eſpied a certain nebuloſe ſtar, larger than any of the reſt, but which he never obſerved there before: In order to examine this new phænomenon, he directed his teleſcope thither, and he immediately found it to be a comet; for, there appeared a very thin nebulous globe with a ſmall bright *nucleus* in the middle: It might likewiſe be ſeen with the naked eye. Beſides the *nebula* or atmoſphere of a comet, it had a ſhort tail, regarding the eaſt, as repreſented Fig. 4. Plate XI.

S. *Bianchini* made his uſual obſervations on this comet, in order to find its longitude, latitude and proper motion.

The firſt night, *viz. October* 17. about 7^h 44′ *p. m.* it paſs'd over the meridian (which almoſt coincides with that of *Rome*) and was diſtant from the zenith 69° 29′.

At 8^h 11′ 30″ the comet was diſtant from *Fomahant* of *Aquarius* 20° 33′; and at 8^h 17′ 30″ from β in *Aquarius*'s right ſhoulder 21° 8′; conſequently, the comet was in 11° 54′ of *Aquarius*, with about 11° 10′ S. Lat. from the ecliptic.

On the 21. it was as near the ſtar ε in the net above *Aquarius*'s left hand, as ε itſelf is near the leſſer ſtar μ in the ſame net, and the comet formed a right line with ε and μ, as repreſented Fig. 5. Therefore, from this obſervation and from the difference of *R. Aſcen.* and declination between the comet and the ſaid ſtar ε, which S. *Bianchini* very carefully obſerved, the place of the comet was in 6° 45′ of *Aquarius* with 8° 5′ N. Lat. from the ecliptic.

Hence likewiſe may be inferred the proper motion and path of the comet; which was thro' the plane of a great circle, interſecting the ecliptic in 9° of *Aquarius*, and forming with the ecliptic an angle of 80° nearly.

The following days it moved always in the ſame proportion and its elongation from the earth encreaſed daily.

After ſeveral trials he could find no ſenſible parallax; conſequently, its diſtance from the earth muſt be very conſiderable.

As to the comet's latitude there was not above a minute or two difference between *Bianchini*'s obſervation, and that made by F. *Carbone* and *Capaſſo*; in all other reſpects the obſervations perfectly agreed.

Obſervations about Waſps *and the Difference of their* Sexes; *by Mr.* Derham. Phil. Tranſ. N° 382. p. 53.

IN *July* 1723. Mr. *Derham* obſerved ſeveral waſps flying about the top of the collegiate *Chapel* in *Windſor Caſtle*, and particularly, frequenting a covering of deal-boards and the pieces of timber, lying on the leads; moſt of theſe waſps were of a larger ſort than uſual, and he imagined they came thither to gnaw the wood, and carry it away in mouthfuls to build their neſts; of which he has taken notice in his *Phyſ. Theol.* particularly, *Book* 4. *ch.* 11. *note* 21. and *ch.* 13. *note* 12: On *July* 6. he obſerved a cluſter only of three waſps, cloſely embracing each other; one of which was a large female waſp; the other two of a leſſer ſort: Soon after he found 8 or 10 waſps, cloſely hanging together, and divers other ſuch like parcels: In the midſt of all which was conſtantly a queen-waſp, and only one; the reſt being always of a different ſort from either the queen or the common waſps, which made him ſuſpect they were male and female; and, therefore, examining another cluſter of them with greater exactneſs, he found the queen-waſp in *coitu* with one of the other waſps, and ſo cloſely joined tail to tail, that it was ſome time before they were parted.

After this he caught all the waſps he could on the top of the chapel, but he could not ſee one of the common labouring waſps among them; but all were for the moſt part male waſps, with now and then a queen or female among them, and ſhe commonly in *coitu*.

Hence it appears, that there are three ſorts of waſps; the queens or females, the kings or males and the common labouring waſps, and each of them very diſtinct.

The queen or female waſp (by many called the king-waſp) is much longer in the body and larger than any other waſp.

The male waſps are leſſer than the queens; but as much longer and larger than the common waſps, as the queen is longer and larger than theſe: The males have no ſtings as the queens and common waſps all have; and theſe are according to

Mouſet,

Mouſet, what authors call Ἀνεντρες, and take to be females; tho' he himſelf be of another opinion, imagining all waſps to have ſtings, from his examining a waſps neſt at *Ham*, *Anno* 1587. in which he found none without a ſting. But Mr. *Derham* is ſurpriſed, how that curious enquirer ſhould miſs of theſe ſting leſs male waſps: He muſt either have been too haſty in his examination, and not being aware of the difference, have thought the males (which are but few in number to the labouring waſps) were the ſame and had ſtings as well as the reſt; or elſe he made his enquiry at a time, when perhaps the males had deſerted the neſt, which, probably, they may do, as the male or drone-bees are forced to do; or elſe the year 1587 (in which *Mouſet* made his obſervation) might produce fewer waſps, at leaſt fewer male-waſps than the ſummer of the year 1723 did, (in which Mr. *Derham* made his) which was obſerved to have a greater abundance of waſp-neſts than had been known for many years: And in all the neſts he ſearched into, he conſtantly found male waſps more or leſs, according to the ſize of the neſt and number of waſps therein; and the part of the neſt, where theſe males are bred, or at leaſt where he found them moſtly reſide, was chiefly the two uppermoſt cells or partings between the combs, but one.

But to return to the diſtinction of the male waſps; another thing by which they may be known from other waſps, is their *antennæ* or horns, which are longer and larger than either thoſe of the queen or common waſps; and with them they ſeem in running to feel more than the others do.

But the chief difference is in the parts of generation of theſe male waſps, which are quite different from thoſe of other waſps.

For the diſcovery of theſe parts, if the *alvus* be preſſed, an horny or ſhell-like part will be thruſt out, of a ſhining black colour, which conſiſts of two parts like ſhells, ſomewhat reſembling the caſtagnets uſed in dancing; at the extremity of each grows a hook, ſomewhat like thoſe of the earwig's tail, but ſmaller; in the middle between theſe hooks appear three parts, the middlemoſt of which is a ſtiff brown tube, very curiouſly made with the fore-part like a ſpoon or ladle, and the other end within the body, is neatly branched and braced to each ſide within the two ſhells abovementioned: A little above which branching is a φυματιον or ſwelling, like that of a dog's pizzle; and probably, ſerves for the ſame uſe, if this tube be (as Mr. *Derham* imagines) the *penis* of the waſp.

On

On each ſide this *penis*, lye two ſtiff parts branched at th top, with ſomewhat like hairs, thereby reſembling bruſhes; a the bottom of theſe are two curious black cells, with an openin on one ſide, like that of the *concha veneris*, and with ſma whitiſh hair growing on one edge thereof. What the uſe c theſe two bruſh-like members may be, Mr. *Derham* coul not tell, unleſs it were to ſtrengthen and ſupport, or direct th *penis* in *coitu*, or provoke therein.

Behind all theſe parts, already deſcribed, and more within the body, lies a long contorted white veſſel, which at firſt Mr. *Derham* took to be the real *penis*, penetrating the ladle-like tube already mentioned: But upon farther examination, he rather takes it to be the ſpermatic veſſel.

The uſe of the 2 little hooks at the extremity of the *europygium* or ſhells, Mr. *Derham* takes to be, the catching hold of the female's *podex*, and directing and aſſiſting the penetration of the *penis* in *coitu*.

As to the parts of generation in the queen or female-waſps, there was nothing to be ſeen ſo remarkable, as in the male; but thoſe parts are very like what we ſee in the common labouring waſps: And with the moſt accurate obſervations Mr. *Derham* could make with his microſcopes, he could not perceive any difference at all. For which reaſon he ſuppoſes it is, that moſt of the writers on waſps and bees have been very confuſed and wavering about the ſexes of theſe two tribes of inſects. It would be endleſs to cite authors and their opinions; eſpecially, concerning the bee-tribe. Mr. *Derham* thinks *Swammerdam*'s opinion, vide *Swam. Hiſt. Inſect.* p. 92. (who as he was one of the firſt that rejected equivocal generation, ſo he was one of the moſt judicious writers on inſects) is the moſt juſt, *viz.* that there are three ſorts of bees. ' 1. The king or more properly ' the queen, as being of the female ſex. 2. The drones, who ' are properly males. 3. The labouring bees, whoſe ſex we ' cannot diſtinguiſh, ſince we can obſerve neither male nor fe- ' male parts in them, which are very diſtinct in the drones or ' kings and the queens, by a vulgar error called the kings. ' According to the incomparable anatomiſt *Joh. Van Horne*, ' there is an *ovarium* in the queens, *&c.*'

As to what is related by *Ariſtotle*, *Pliny*, *Virgil*, or any other ancient authors, or by our more numerous modern authors, concerning the production of waſps out of horſes; or of bees out of oxen or young bullocks; as alſo of their polity, their emperors, kings, dukes and common ſubjects, their exact

diſcipline

ſcipline and juſtice, their ſtrict temperance and other virtues, ith a great deal more, is ſo very whimſical, that it is not orth while, to take any farther notice of it: But there is a ory told by *Mouſet*, in *Theat. Inſect. l.* 1. *c.* 8. that deſerves ır conſideration, *viz.* 'that in 1582, being on the higheſt ridges of the *Cartmel-hills* (Mr. *Derham* ſuppoſes in *Lancaſhire*) he obſerved among the rocks two ſpecies of waſps deſperately fighting; that they differed only in ſize; that the larger truſted to their ſtrength, and the ſmaller to their numbers; there being ſix of the ſmaller ones engaged againſt only one of the larger ſize; and that the battle was not in the air, but among the graſs, and laſted for ſome hours in the hotteſt ſun, it not being at an end in two or three hours.' The cauſe this engagement according to *Mouſet* was, that the large aſps are wont to rob the leſſer of their honey and young, or o them ſome ſuch injury; and the leſſer being very revengeful nd naturally full of courage, did outbrave even *Mars* himſelf assaulting their enemy. But this engagement Mr. *Derham* kes to be ſuch another, as that he has given the account of, amely, one under the conduct of *Venus*, rather than of *Mars*. And as there is no doubt to be made of its being ſuch, and nat the engagement ſeen by *Mouſet* was on the higheſt tops of *artmel* (*in ſummis Cartmeli Montium jugis*) as that Mr. *Derham* ſaw was on the very top of *Windſor-Chapel*, it may deſerve be conſidered, whether waſps ever copulate in lower places, bvious to diſturbance and every one's obſervation; or only on ich eminencies, where they can be more out of ſight; and conſequently, in greater ſafety: And if at any time they ſhould be ound in copulation, they may all with ſafety be ſeiſed with the aked hand, provided it can be ſecured againſt the queen-waſp, hich is the only one in the company, that is provided with a ing.

To conclude theſe obſervations about the ſexes of waſps, Mr. *Derham* takes notice of *Mouſet*'s experiment, (which the ormer tried) *viz.* if you take a waſp by the feet and ſuffer her o buz, thoſe waſps, which have no ſtings, but none of thoſe nat have, will fly to her; which ſome, ſays he, uſe as an arument to prove, that ſome waſps are males, and ſome females. This experiment Mr. *Derham* was minded to try with a queenvaſp more eſpecially; not knowing, but that waſps, particularly the males, might be as fond of their queens, as the bees re of theirs, who will not forſake them, but live and die with hem. But he did not find it ſucceed ſo among the waſps: For,

For, tho' he put ſome queen-waſps and others alſo, near the entrance of ſome large waſp-neſts; yet he did not obſerve any flock near them, only now and then one of the common waſps for a little while, to ſee their fellow confined: But, it is true the queen-waſps he confined, were weak and did not buz long; as alſo the time of copulation was, probably, paſt, it being *Auguſt* 12. when he tried the experiment.

Two Curious Obſervations on the prodigious ſize of the Omentum *and uncommon Colour of the* Saliva; *by Dr* John Huxham. Phil. Tranſ. N° 382. p. 60. *Tranſlated from the* Latin.

A Certain woman was for a long time troubled with colic and vomiting, upon which there enſued a hard ſwelling of her belly, that, increaſing daily, grew to a prodigious ſize. And now the patient vomited every thing, black and pure bile, and at length the *fæces* themſelves were thrown up. The pain being often very ſevere, eſpecially in the left *hypocondrium*, fomentations, cathartics, glyſters, anodynes and ſeveral other medicines were made uſe of; but all to no purpoſe; the pain continued, the ſwelling increaſed, and was as hard as a piece of board. From this ſwelling there aroſe ſeveral protuberances, as it were, one or two of which were as big as a child's head, and others as big as a man's fiſt; the largeſt was in the left *hypocondrium*, where ſhe felt the moſt exquiſite pain. As the ſwelling increaſed, the patient had a great difficulty of breathing; and after ſhe had ſuffered a deal of pain for almoſt 14 months ſhe at length died.

The belly was obſerved to be very protuberant, while the reſt of the body was exceedingly emaciated. Upon cutting the *abdomen*, there preſented to view a large maſs of ſuet, as it were, (only not ſo white) that filled up the whole cavity of the belly in ſuch a manner, that neither the ſtomach, liver or any of the inteſtines, could be ſeen; for, this enormous *omentum* adhered to the *peritonæum* in ſeveral places; eſpecially, in both the *hypocondria*, where the adheſion was broader and firmer. Upon making a crucial inciſion on the *peritonæum* and ſeparating it from the *omentum*, this latter adhered to the whole concave ſide of the liver; the ſtomach was oppreſſed by its weight, as were alſo the *duodenum*, *colon* and *jejunum*; it adhered and was confounded, as it were, with the fatty integument of the kidneys, eſpecially the left; ſo that the *colon*, a little higher than where it terminates in the *reƈtum*, was entirely wrapped up in this con-

creted

creted mass of suet: Hence the passage of the *fæces* was obstructed, and hence proceeded those exquisite pains the patient was tormented with before her death: And to observe this by the bye, for several days before she died, she had no stool either spontaneously or by the sharpest glysters.

Afterwards the *omentum* was separated from the liver, stomach, schirrous pancreas, the intestines, and at length from the mesentery, and the internal *lamina* of the *peritonæum*, that covers the kidneys: The spleen was sunk in some measure into this mass, being thereby contracted and resembling a piece of hide.

This *omentum* weighed 16 *lb.* and ½ averdupois, besides one or two pounds more that adhered to the parts. A prodigious weight! If we consider that the *omentum* of the fattest man scarcely weighs 3 pounds; in a word the Dr. observed none larger, in all the subjects either he himself dissected, or saw dissected by others. It is true there is a much larger *omentum* mentioned by *Gregory Horstius* in his *Obs.* and also in the *Ephemerid. German. Ann.* X: Yet still this may be justly reckoned a prodigious one.

Both in the external and internal substance of this *omentum*, the Dr. observed a great many blood-vessels, exceedingly dilated; some of which were bigger than a goose-quill, and others terminated in aneurisms, as it were: Out of the largest of which he expressed about six ounces of blackish blood, with some whitish *grumæ*; whether these were particles of fat, absorbed by the epiploic veins, and concreted by the distemper, the Dr. could not tell.

This mass seemed to consist of a great many lobes, closely adhering to each other; yet he separated some, of which a few were as big as a small apple, and of much the same shape, the middle was harder than the rest, and could hardly be cut with a knife.

In this subject were also the following remarkable particulars; the lower part of the liver was become schirrous; several *calculi* were taken out of the gall-bladder, of the colour of pit-coal, friable, and floating on water, not improperly called shining bile; for, there were a great many sparkling (undoubtedly saline) particles therein.

The glands of the mesentery were schirrous; nay, some of them almost as hard as a stone; the thin guts were inflamed; the *colon* together with the *cæcum* was almost entirely gangrenous, as also the vermicular process.

Both the kidneys were pretty ſound; the right ſent out two *ureters*, and upon diſſecting the kidney, the Dr. eaſily perceived the reaſon; for, the pelvis of the kidney was divided by a kind of *ſeptum*, a finger thick, entirely of the ſame ſubſtance with the reſt of the *parenchyma* of the kidney; ſo that there was a double kidney, as it were, with a double *pelvis* and a double *ureter*.

There were about two pounds of bloody *ſerum* in the lower part of the *abdomen*.

This diſorder ſeem'd to be entirely incurable, unleſs it could have been prevented at firſt.

Mr. *Fox*, 40 years of age, of a thin, bilious temperament, languiſhed for a month or two with a *nauſea*, jaundice and colic: At length being ſeiz'd with a violent fit of the colic, from drinking plentifully of cyder, he vomited every thing, made but little water, of a lixivious colour, which depoſited much greeniſh ſediment.

The Dr. immediately order'd an emetic potion of *Ipecacuanah*, upon which drinking plentifully of an infuſion of *C. Bened.* he brought up a great deal of viſcous bilious matter; and then by giving him a glyſter of *terebinth.* he had two or three ſtools; upon taking an anodyne mixture, his vomiting and gripes ceas'd; and now the patient complain'd of an extreme langour, and a diſtention of the *abdomen*; and a little after the pain return'd. The Dr. preſcrib'd a *bolus ex jalap.* ℈j. *calomel. gr.* viii. *ſpec. diambr. gr.* vi. *laud. ſolid. gr.* i. *ſyrup. de ſp. cerv. q. ſ.* to be taken with *tinct. ſacr.* ʒii. Upon this all was quiet again, and 12 hours after, he had three ſtools of a very thin bilious matter: Upon taking an anodyne draught he had a quiet night; in the morning complaining of a pain and ſwelling in his jaws, he ſpit a thick duſkiſh matter; and ſoon after he diſcharg'd a large quantity of a green *ſaliva*, very much reſembling porraceous bile, only thinner. This flux of green *ſaliva* laſted about 40 hours, in which time he diſcharged upwards of two pints; then it inclined to a yellow colour, till at length it became like a ſolution of *gut. gambæ*, the quantity rather increaſing than diminiſhing.

This colour likewiſe continued for 40 hours, and then the *ſaliva* became gradually pellucid; and the ſalivation, as it had begun, ceas'd all of a ſudden.

In two years time he had been troubled twice or thrice with the jaundice, before the acceſſion of this diſorder: Ten years

years before, he had been in great danger of his life by an extraordinary ſalivation, which came on ſpontaneouſly; and then he had not taken the leaſt grain of mercury, nor was his *ſaliva* any ways coloured.

In this caſe the following things are worth obſerving; firſt, a ſpontaneous ſalivation, or probably, raiſed by ſo very ſmall a quantity of *calomel*; for, the Dr. had preſcrib'd ſeveral doſes of the ſame *calomel*, and ſometimes to the quantity of a whole ſcruple, without the leaſt ſign of a ſalivation.

The next thing to be obſerv'd is the uncommon colour of the *ſaliva*; for, the teeth and jaws were tinged green, as if they had been done over with verdigreaſe, and they continued of that colour for 14 days after the patient recovered.

It is farther to be obſerv'd, that this flux was critical, and indicated the jaundice and colic; for, as ſoon as the ſalivation began, the patient had not the leaſt pain in his belly, and the greeniſh colour of his ſkin gradually diſappear'd; he likewiſe made water in greater quantity, but of a blackiſh colour; and tho' before this flux he had been very weak, and almoſt dying, yet he bore well this extraordinary diſcharge of *ſaliva*.

It is well known that the *ſerum* of icteric patients is tinged with the bile; nor need it ſeem more ſurpriſing, that a yellow *ſerum* ſhould be diſcharged thro' the ſalivary glands, than thro' the ſerous veſſels of the *cutis*, as is frequently obſerv'd to be the caſe, after firſt applying a bliſter, not to mention bilious urine. But it is not ſo eaſy to determine whence this green colour of the *ſaliva* ſhould proceed: The Dr. however takes the drinking of the cyder to have been the procatartic cauſe.

A green colour is produced upon mixing an acid with the bile: Hence the green ſtools of children, attended with gripes, by the ſouring of the milk upon their ſtomachs, and hence ariſes a porraceous bile: Now the acid of the cyder being abſorbed by the lacteals, or probably by the meſeraic veins, the conſequence would be a change of the yellow colour of the *ſerum* into a green: He owns, indeed, that acids, nay, that of vitriol, are changed by the *vis vitalis* of the body into an alcalious nature: But when the ſtrength fails, and the concoction is vitiated, and the *lympha* becomes effoete, and the bile unactive, to which if you add the ſlow circulation of the blood, the acids, being but very little alter'd, are diveſted of their acid nature, neither in the firſt paſſages,

nor in the blood itſelf. This is confirm'd by the acid ſweats of weak perſons, and the acid taſte in the mouths of ſuch as recover from inflammatory fevers, who have had large quantities of acid liquors given them to drink, the Dr. himſelf ſeveral times obſerv'd, that ſoon after drinking plentifully of cyder, he made water that ſmelt ſtrongly of it.

The Dr. waited of a woman, taken with a violent hemorrhage that laſted for ſome time, to whom among other things was given a great deal of the *elix. vitriol. Mynſich.* At length the hemorrhage ceas'd; but the patient, being thereby weakened, fell into ſlight ſpaſms, had pains in her joints, the gripes with a *diarrhœa*; ſhe made ſeveral green ſtools, very like cows dung, for ſeveral days together, tho' ſhe had had three doſes of *rhabarb.* to carry off the acid particles, adhering to the inteſtines: This acid ſalt, mix'd with the blood, was ſeveral days after thrown off into the inteſtines: without undergoing any alteration from the *vis vitalis.*

M. *Leewenhoeck* obſerv'd with his microſcope, that the acid ſalts of the chyle are blunted and comminuted by the bile; but when there is an obſtruction in the liver, acids paſs thro' the lacteals entire: That there was an obſtruction of the bile in the former patient, was evident from his conſtipation, and white ſtools.

The Dr. thinks his hypotheſis may be illuſtrated, if we conſider the cauſe of the green colour of cachectic virgins: For they greatly delight in unripe fruits, vinegar, *&c.* but the organs of concoction being vitiated, they mix with the blood without undergoing little or no alteration, and overcharge both it and the bile with acids: Hence the glands are obſtructed, the face from being green becomes pale, and hence a dropſical tumour; and what is more, the ſchirrous liver of ſuch as have died of the green ſickneſs, has often been obſerv'd to be green.

Now the Dr. takes the yellow colour of the *ſaliva* ſucceeding the green to be owing to this, *viz.* that after the acid had been partly thrown off from the blood, and partly deſtroy'd by the increaſe of its motion, by the adminiſtering of cardiacs, there only remain'd the ſuperfluous bile to be thrown off, which ſoon happened upon opening the pores of the bile, being in a ſurpriſing manner diſcharged by the wide ſalival ducts.

The Effects of Inoculation in New England; *the Eclipse of the Sun in* November 1722; *and the Venom of Spiders; by Dr.* Robie. Phil. Tranſ. N° 382. p. 67.

THEY had not hitherto obſerv'd any ill effects of inoculation in *New England*; but the inoculated were as well, and ſome of them a great deal better than ever: As to the bad conſequences that have been obſerv'd in *England*, Dr. *Robie* could hardly think they were the genuine effects of inoculation, but rather owing to ſome previous diſpoſition to other diſtempers, or for want of due evacuations after inoculation, and too ſoon healing the places of inciſion.

As to the great eclipſe of the ſun in *November* 1722, the Dr. had an account, that the obſervers at *Yale* College in *Connecticut* Colony ſuppos'd it to be but about eight digits: whereas by his calculation for *Cambridge* it was to have been about 11 digits. *Yale* college lies about 8 or 10 minutes weſt from *Cambridge*, in about 40° and ½ N. Lat. The Dr. did not calculate it for that place, but for *London*, and it agreed to about 7 or 8′ with Mr. *Derham*'s obſervations, and was pretty exact as to the digits. The gentleman who wrote the Dr. word of this, tells him he calculated it, and could make it ſcarce 7 digits eclips'd; and that it went off from *America* into the ſea, a great deal more ſoutherly than the Dr. made it: For by his calculation it was to be centrical at *Cape Cod*, and ſo to paſs over to the iſles of *Cape de Verd*: But taking this gentleman to be very much miſtaken, the Dr. proceeds to give his obſervations, as far as he was able to make them; and he thinks they were accurately made.

	h. ′	
Nov. 27. 1722.	7 27	in the morning he obſerv'd the ſun riſe eclips'd on its upper *vertex* to the ſouth, about four digits; tho' ſome on the top of the new college obſerv'd it 2 or 3′ before. The ſun's true ariſing this morning was 7h. 30′: Hence the refraction is about 6′, and ſo he often obſerv'd it. From this time till about 8h. 30′ or 40′, he ſaw no more of the ſun; but then he judges it was eclips'd ſix digits or more.

h. ′ m

h.	′	″	
8	55	15	The ſun was eclips'd 4 digits and ¾ nearly.
9	00	16	Four digits and ½.
9	19	45	A little ſpot in the ſun emerged.
9	25	45	He obſerv'd the moon go off the ſun.
9	25	45	Mr. *Danforth*, in a room juſt by the Dr. obſerv'd the ſhadow go off the paper about 30° from its lower *vertex* to the eaſt.
9	25	20	Mr. *Appleton* obſerv'd the ſhadow go off the paper, fix'd to the college braſs quadrant at his houſe. Mr. *Owen Harris*, an ingenious ſchoolmaſter in *Boſton*, ſays, he obſerv'd the end at about 26′ after 9.

By the ſecond obſervation the ſun's diameter was to the moon's as 1000 to 972; by the third as 1000 to 975. The eclipſe was obſerv'd at *Boſton*, allowing for its diſtance, as the Dr. obſerv'd it at the college; and at *Barnſtable* on *Cape Cod*, there was but a little left of the ſun; and nearer the head of the cape there was a ring of light quite round the moon.

The teleſcope the Dr. made his obſervations by was 24 foot long; that Mr. *Danforth* made uſe of, thro' which the rays were tranſmitted, was eight foot, and the braſs quadrant the very ſame Dr. *Halley* made uſe of at St. *Helena*. The Dr. took the ſhadow to paſs off *America* about *Cape Cod*, ſuppoſing its Lat. to be 40° N. or 40° 10′, and about 10 or 15′ E. from *Cambridge*.

Sep. 13. 1722. One *Nat. Ware* of *Needham* was bit by a ſmall ſpider (which he could not give an exact deſcription of, having cruſhed it to pieces between his ſtocking and leg) the account he gave the Dr. was as follows, *viz.* That getting up early in the morning, and putting on his ſtocking, he immediately felt ſomething bite his left leg, a little above his ancle; about half an hour after, he felt a pain in that leg, and about half an hour from his firſt perceiving pain in his leg, he felt a pain in his groin, and at the ſame time a creeping pain in the calf of his left leg; and about an hour after, it got into the ſmall of his back, and then round him, and in his ſtomach, and in his right thigh, and afterwards he had a numb-

numbneſs in his head; the pains were not conſtant and fixed, but erratic and very acute; his pulſe was very low and heavy.

September 14. In the morning the patient came to ſee the Dr. and was much better, tho' he had but little ſleep in the night. The means uſed were only *Sp. Cor. Cerv.* and *Sal. Vol. Corn. Cerv.* with *Vin. Viperin.* and onions or garlick externally applied to the place, where the wound was. Theſe things raiſed his pulſe, and ſo, the Dr. ſuppoſes, aſſiſted nature to throw off the venom.

Several Obſervations made in Italy *of a* Lunar Eclipſe, *which happened* September 8. 1718. Phil. Tranſ. N° 382. p. 71. *Tranſlated from the* Latin.

THE following obſervations were made in the houſe of S. *Pietro Bembo*, at *Padua*, by S. *Giovanni Poleni* and *Giovambatiſta Morgagni*.

app. time. h.	p. ′	m. ″	
			About the beginning of the eclipſe clouds covered the moon.
6	54	25	The ſhadow comes to the eaſtern part of *Mare humorum*, is diſtant from *Ariſtarchus* the diameter of that *macula* and at the ſame diſtance from *Kepler*.
7	5	5	It comes to *Copernicus*.
	12	56	It comes to *Plato*.
	22	31	It entirely covers *Manilius*.
	30	55	It begins to cover the eaſtern part of *Mare Nectaris*.
	41	53	It comes to the ſecond ſuperior eaſtern part of *Mare Criſium*.
	46	58	The *penumbra* reaches the extremity of the diſk.
	49	4	An almoſt total immerſion.
			All the time of the immerſion the moon is obſerved of a duſkiſh red colour: Juſt after the immerſion, that part of the moon towards the eaſt is more obſcured than the reſt.
	33	3	That part of the moon near the middle of the diſk is more obſcured; and the extreme parts of the diſk, quite round, are leſs obſcured.

A little

app. time. h.	*p.* ′	*m.* ″	
9	30	49	A little ſtar, that could not be diſcerned by the naked eye, ſeems to be ſcarce 10 ſeconds diſtant from the moon's diſk, over againſt *Lansbergius*.
	32	9	The *penumbra* becomes clear in the eaſtern extremity of the diſk.
	36	4	The beginning of the emerſion towards the eaſt.
	40	39	Now *Grimaldus* emerges out of the ſhadow.
	44	38	An occultation of the above obſerved little ſtar by the moon, tho' uncertain.
	49	34	*Gaſſendus* emerges.
	50	46	*Mare humorum* entirely emerged.
10	00	3	*Copernicus* emerges.
	5	55	*Plato* begins to emerge.
	14	41	*Eudoxus* emerges.
	19	12	*Menelaus* emerges.
	27	12	*Mare Nectaris* entirely emerged.
	36	28	The ſhadow biſſects *Mare Criſium* by its greater diameter.
	39	12	The ſhadow begins to be ſomewhat rarer.
	41	2	The apparent end of the ſhadow.
	42	57	And that of the *penumbra*.

The following obſervations were made in the palace of the *Iſtituto delle ſcienze*, at *Bologna*; by S. *Geminiano Rondelli*, *Giuſeppe-Antonio Nadio* and *Giulio Cæſare Pariſi*.

true time h.	*p.* ′	*m.* ″	
			The beginning of the eclipſe was not obſerved.
6	51	36	*Mare humorum* at the ſhadow.
	56	22	*Capuanus* at the ſhadow.
	56	37	*Mare humorum* entirely immerged.
7	1	7	*Bullialdus* at the ſhadow.
	2	52	Entirely immerged.
	3	37	*Copernicus* entirely immerged.
	11	22	*Tycho* at the ſhadow.
	12	52	*Tycho* entirely immerged.
	15	37	*Plato* at the ſhadow.

Plato

app. time. h.	p. ′	m ″	
7	16	27	*Plato* entirely immerged.
	19	22	*Manilius* at the ſhadow.
	19	52	*Mare ſerenitatis.*
	23	57	*Mare tranquillitatis.*
	35	8	*Meſſala.*
	36	8	*Meſſala* entirely immerged.
	36	38	*Mare fœcunditatis* at the ſhadow.
	37	23	*Promontorium Somni.*
	39	23	*Cleomedes.*
	39	53	*Mare Criſium.*
	44	8	*Mare fœcunditatis* entirely immerged.
	44	43	*Mare Criſium* entirely immerged.
	47	18	The total obſcuration of the moon, according to S. *Nadio*'s eſtimation.
	47	53	The total obſcuration, according to S. *Pariſi.*
9	33	40	The beginning of the emerſion of the moon.
	36	35	*Grimaldus* entirely emerged.
	40	54	*Galilæus* emerged.
	42	34	A certain conſtellation is covered by the moon, in the ſame vertical nearly with her centre.
	47	50	*Mare humorum* emerged.
	52	10	*Bullialdus* emerged.
	54	25	*Tycho*'s centre emerged.
	55	12	*Tycho* entirely emerged.
	58	46	*Mare Nubium* emerged.
10	4	2	*Plato* at the termination of the ſhadow.
	5	33	*Plato* entirely emerged.
	17	12	*Inſula ſinus medii* emerged.
	23	47	*Meſſala* at the termination of the ſhadow; and at the ſame time *Mare ſerenitatis* entirely emerged.
	27	58	*Mare tranquillitatis* emerged.
	30	12	*Cleomedes* emerged.
	32	8	*Mare Criſium* touches the extremity of the ſhadow.
	34	7	*Mare fœcunditatis* entirely emerged.
	36	19	*Mare Criſium* entirely emerged.
	37	39	The end of the eclipſe.

The following obſervations were made in the ſuburbs of *Bologna*, ſouthwards by S. *Euſtachio* and *Gabrielo Manfredi*.

app. time. h.	*p.* ′	*m.* ″	
6	31	48	Now the moon begins to riſe out of the hills tinged with the *penumbra* of the atmoſphere.
	42	13	The beginning of the true eclipſe, as far as could be judged by the uncertain termination of the ſhadow.
			A little after the moon was covered by clouds and intercepted by trees.
	52	48	The ſhadow extends over *Ariſtarchus* and *Keppler*, and at the ſame time ſeems to touch *Mare humorum*.
7	2	23	The ſhadow paſſes thro' the middle of *Bullialdus*, and at the ſame time touches *Copernicus*.
	4	2	The ſhadow paſſes thro' the middle of *Copernicus*.
	5	4	*Copernicus* entirely covered.
	7	5	The ſhadow reaches *Pitatus*.
	10	54	———————— *Tycho*.
	12	19	The middle of *Tycho* covered.
	13	9	*Tycho* entirely covered.
	15	34	The ſhadow at *Plato*.
	16	7	At the middle of *Plato*.
	16	54	*Plato* entirely covered.
	20	9	*Manilius* covered.
	20	34	The ſhadow touches *Mare ſerenitatis*.
	23	44	*Menelaus* covered.
	24	56	*Dionyſius* covered.
	27	34	*Plinius* covered.
	29	49	The ſhadow at *Catharina*, *Theophilus*, and *Cyrillus*.
	30	36	The ſhadow touches *Fracaſtorius*.
	31	44	The middle of *Fracaſtorius* covered.
	32	34	*Promontorium acutum* immerged.
	35	15	*Promontorium Somni*.

app

app. time. h.	p. ′	m. ″	
	37	57	*Taruntius.*
	39	39	The ſhadow touches *Mare criſium.*
	42	16	It paſſes thro' the middle of *Mare criſium.*
	44	5	*Mare criſium* entirely immerged.
	47	50	The total immerſion of the moon.
			All the time of the eclipſe the moon was very diſtinctly ſeen in a clear ſky, and of a reddiſh colour, denſer, where it was deepeſt immerged into the ſhadow.
3	38	50	At this time, and for ſome minutes after, the face of the moon appeared equally obſcured on all hands: Whence it was plain, the eclipſe was nearly central.
3	27	50	Over againſt *Grimaldus*, where the emerſion was to happen, there began to appear a remarkable brightneſs.
	29	20	The beginning of the emerſion doubtful.
	33	20	The emerſion undoubtedly began.
	35	21	*Grimaldus* began to emerge.
	35	55	The center of *Grimaldus* emerges, and now *Riccioli* entirely emerged.
	36	26	*Grimaldus* entirely emerged.
	39	28	*Galilæus* emerged.
	41	22	The ſhadow touches *Mare humorum.*
	42	31	The little ſtar, which continued for ſome time near the inferior limb of the moon, was now at length hid by the moon, about *Tycho*, that was ſtill eclips'd: Other little ſtars about to be hid under the moon; but after that one or two digits of the moon's diſk began to ſhine, all vaniſh'd by reaſon of its brightneſs.
	43	53	The ſhadow paſs'd thro' the middle of *Mare humorum.*
	45	41	*Ariſtarchus* emerges.
	47	23	*Keplerus* emerges.
	52	6	*Bullialdus* emerges.
	53	31	*Tycho* begins to emerge.
	54	9	The middle of *Tycho* emerges.

app. time. h.	p. ′	m. ″	
	55	1	*Tycho* entirely emerged; at which time the little ſtar had not yet emerged out of the moon.
	55	21	*Copernicus* begins to emerge.
	56	6	The middle of *Copernicus* emerges.
	59	15	*Copernicus* entirely emerged.
10	00	1	The little ſtar which a little before was hid under the moon, was now obſerv'd at ſome diſtance from her limb, and ſeem'd as if it had emerged four or five minutes before: it was over againſt the obſcure part of the moon's lower limb, not far from the termination of the ſhadow.
10	4	51	The ſhadow paſs'd thro' the middle of *Plato*.
	5	36	*Plato* entirely emerged.
	13	6	*Manilius* emerges.
	16	31	*Dionyſius* emerges.
	16	41	*Menelaus* emerges.
	20	31	*Fracaſtorius* entirely emerged.
	23	51	*Snellius* and *Furnerius* entirely emerged.
	24	5	*Promontorium acutum* emerged.
	25	11	*Meſſala* entirely emerged.
	31	11	*Proclus* emerged.
	31	51	*Mare criſium* begins to emerge.
	34	3	The middle of *Mare criſium* begins to emerge
	36	7	*Mare criſium* entirely emerged.
	38	5	About this time the true ſhadow ſeemed to have gone off the moon's diſk, while the *penumbra* continued thereon a conſiderable time.

In the *Ephemerides*, publiſh'd in 1715, from M. *Caſſini*'s tables, for the uſe of the *Iſtituto Bologneſe delle Scienze*, the beginning of theſe eclipſes was mark'd at 6h. 41′: The total immerſion at 7h. 46′, the begining of the emerſion at 9h. 33′, the end at 10h. 38′, which times ſcarcely differ one or two minutes from the times obſerv'd.

The following obſervations were made by the Marquis *Antonio Ghiſilieri*, on the obſervatory in his houſe at *Bologna*

app. time. h.	p. ′	m. ″	
6	40	23	The beginning of the eclipſe doubtful.
	51	23	*Mare humorum* at the ſhadow.
	55	46	*Capuanus* at the ſhadow.
7	1	13	*Bullialdus* at the ſhadow.
	28	14	*Mare nectaris* entirely immerged.
	32	30	*Promontorium acutum* at the ſhadow.
	36	45	*Promontorium ſomni* at the ſhadow.
	38	45	*Mare criſium* at the ſhadow.
	46	37	The total immerſion of the moon.
9	33	50	The beginning of the emerſion.
	35	39	*Grimaldus* entirely emerged.
	54	17	*Tycho* entirely emerged.
10	15	6	*Plinius* entirely emerged.
	32	39	*Mare criſium* begins to emerge.
	37	42	The end of the eclipſe.

Experiments and Obſervations on the freezing of Water *in* vacuo; *by M.* Fahrenheit. Phil. Tranſ. N° 382, p. 78. *Tranſlated from the* Latin.

AMONG the ſeveral ſurpriſing phænomena of nature M. *Fahrenheit* always thought the freezing of water not the leaſt conſiderable: Hence he was often deſirous of trying what would be the effects of cold, upon including water in *vacuo*: And becauſe the ſecond, third, and fourth of *March* 1721 O. S. were favourable for that purpoſe, he made the following experiments and obſervations.

Before he comes to the experiments themſelves, he premiſes ſome things on his thermometers, the diviſion of the ſcale, and his method of exhauſting them. His thermometers were chiefly of two ſorts; one fill'd with ſpirits of wine, the other with mercury; and their lengths were different, according to the uſes they were deſign'd for; but they all agreed in having the ſame number of degrees, and in the fixed limits of their variations. The ſcale of thermometers for meteorological obſervations only, begins below at 0 and ends at 96 degrees. There are three fix'd limits in this diviſion; the firſt is in the loweſt part or beginning of the ſcale, and produced by the commixture of ice, water and ſal-armoniac or even ſea ſalt; if into this mixture you put the thermometer, it deſcends to 0; this experiment ſucceeds better in winter than in ſummer: The

ſecond

ſecond limit is when water and ice are mix'd together without the above-mentioned ſalts; for, putting the thermometer into this mixture, it is at 32 degrees, which M. *Fahrenheit* calls the limit of initial congelation: For, ſtagnant waters are cover'd with a thin cruſt of ice in winter, when the thermometer is at that degree: The third limit is at the 96th degree, to which the ſpirits are dilated, while the thermometer is held in the mouth or under the arm-pits of a healthful perſon, till it perfectly have acquir'd the ſame degree of heat with the body. But if we want to find the heat of a man in a fever or any other hot diſtemper, we muſt make uſe of another thermometer, whoſe ſcale extends to 128 or 132 degrees: But whether theſe degrees are ſufficient for finding the greateſt degree of heat of any fever, M. *Fahrenheit* had not hitherto experienced; yet it is ſcarce credible, that the degree of heat of any fever ſhould exceed the above-mentioned number of degrees. The ſcale of the thermometers for determining the degrees of heat in boiling liquors, does alſo begin at 0 and contains 600 degrees; for, the mercury itſelf (with which the thermometer is fill'd) begins to boil at about the ſame degree.

That the thermometers may be the ſooner affected with all the changes of heat, they are provided with glaſs cylinders inſtead of balls: For, this, by reaſon of the greater quantity of ſuperfices, they are ſooner penetrated by the different degrees of heat.

M. *Fahrenheit's* method of exhauſting was thus; a glaſs ball (as repreſented Fig. 6. Plate XI.) furniſhed with a ſmall tube B C three or four inches long, and tapering at the extremity C, is heated over a fire; after which the extremity of the tube is immerſed in water, till by the cooling of the air, contain'd in the ball, it become fill'd with ſome drops of the water; and then it is held with a pair of tongs over the broader flame of a lamp, or over live coals, till the water in the ball begin to boil, and the vapour forcibly burſt out: This boiling is continued for ſome time, after which the ball is remov'd from the fire, and the flame of a candle applied to its extremity. As the ball cools, the vapour rarified by the fire, is ſucceſſively condens'd, and its egreſs gradually diminiſh'd; and as ſoon as it entirely ceaſes, the extremity of the tube melts, and the ball is hermetically ſeal'd and exhauſted. Whether all the air contain'd therein be well exhauſted in this manner may be tried, by breaking the extremity of the tube

under

under mercury; for, then the ball will be entirely fill'd with mercury, if it be carefully broken without admitting the external air: You may alſo break it under water; but tho' it be done never ſo carefully, the ball will not be compleatly fill'd with water: For, while the water enters the exhauſted ball, the air which is always mix'd with water, ſeparates from it into very ſmall bubbles, which after uniting, form a larger one in the ball: In the ſame manner the ball may be exhauſted, if the third, the half, or a greater part of the ball be requir'd to be fill'd with water: For, it is firſt fill'd with the deſired quantity of water, and then, after boiling the water, it is hermetically ſeal'd.

The experiments were as follows; *March* 2. 1721. he expos'd to the cold a glaſs ball, about an inch in diameter, exhauſted in the manner above-mentioned; and fill'd with rain-water almoſt half full: The temperament of the air was in the thermometer mark'd at 15 degrees. In an hour after, he found the water ſtill fluid in the ball, the reaſon of which he took to be, that the cold had not thoroughly penetrated it: In order to remove all doubt, he left the ball, expos'd all night in the open air. Next day, *viz.* the third of *March* at 5 o'clock in the morning he found the water ſtill fluid, and the thermometer at the ſame degree; the cauſe of which unexpected phænomenon he attributed to the abſence of the air. In order to diſcover the truth of this conjecture he broke the extremity of the tube, that the exhauſted ball might be again fill'd with air; upon which, the whole maſs of water was all of a ſudden mix'd with very thin *lamellæ* of ice. Before he repeated the experiment, he would try by another, whether theſe icy *lamellæ* would float in the water; wherefore he broke the ball, and putting ſome of the ice into ſome water in a glaſs cup, he obſerv'd it float therein.

Viewing the cup again, he in a little time obſerv'd all the water mixed with icy *lamellæ*; yet the greateſt part of the water ſtill continued fluid between the interſtices: The thermometer, put into this mixture, ſtood at 32 degrees. M. *Fahrenheit*, being deſirous to view this phænomenon more attentively, reſolv'd to repeat the experiment with two other balls, and after preparing them in the manner above-mentioned, he expos'd them for an hour in the open air, the thermometer was then at 20 degrees: An hour after, he found the water ſtill fluid in both the balls; but after that the exhauſted ball was again fill'd with air, the water (as in the former experiment)

was

was very ſoon mixed with icy *lamellæ*; and their production was ſo inſtantaneous, that it could hardly be obſerv'd with the eye: And becauſe the production of the *lamellæ*, in the glaſs cup, had eſcap'd his obſervation, he was ſtill very deſirous to view that phænomenon with ſomewhat more attention. Before he broke one of the balls, he ſeparated the water in the ſaid cup from the icy *lamellæ*; upon which he broke the ball, and threw the ice into water; the ice, it is true, floated on the water, but he in vain expected the production of the *lamellæ* in the cup: M. *Fahrenheit* poſtpon'd the further trial of theſe experiments till 11 o'clock at night, when he expos'd three balls in a keen froſt: Two of theſe balls were again fill'd with water about half full; and the third ball was fill'd about $\frac{3}{4}$ parts full: The temperament of the air was by the thermometer mark'd at 26 degrees; at 4 o'clock in the morning he found the temperament of the air the ſame by the thermometer, and the water in the two balls, which were only fill'd half full, ſtill fluid; but in the third ball, the water was frozen and the ball broken: The ice was intermixed with very ſmall bubbles, but few in number, and its tranſparency was very much interrupted, and reſembled the confus'd cryſtallization of ſome ſalts. M. *Fahrenheit* attributed this to ſome imperceptible fiſſure, by which the external air had inſinuated, and ſo produced the congelation of the water.

M. *Fahrenheit*, being ſtill very deſirous of viewing the production of the *lamellæ* in the glaſs cup, took the ſaid veſſel into the room where the former experiments were made, and happening to ſtumble a little, the water in the glaſs was agitated, and in that very inſtant the whole maſs appear'd intermixed with ſeveral icy *lamellæ*: By this accident he diſcover'd, that ice could be produced in pretty cold water by agitating it; and this put him on trying whether water would freeze in *vacuo* by agitation. Therefore, after ſhaking the ball a little, he had the pleaſure of obſerving the ſame phænomenon, and at the ſame time found his miſtake in attributing the fluidity of the water to the abſence of the air. In the mean time he found by the thermometer, that the froſt was much abated; for, it now ſtood at 28 degrees; and therefore he ſoon diſſolv'd the ice with his hand, and again expos'd one of the balls in the open air, where leaving it about half an hour, he obſerv'd the froſt remit ſtill more; for now the thermometer ſtood at 32 degrees; and becauſe he

was

was afraid, that by the remitting of the cold, it would be to no purpoſe to repeat the experiment, tho' the ball were left expoſed longer in the open air; he at that very time attempted to produce a congelation in the water by ſhaking the ball; but tho' he ſhaked it ſtrongly, not the leaſt ſigns of it appeared. Having no hopes of producing it in this manner, he ſtill had a mind to try, whether it would ſucceed, if the exhauſted ball were again filled with air. Breaking therefore the extremity of the tube, very minute *ſpicula* of ice were diffuſed thro' the whole maſs of water, which, by ſhaking it round, mounted to the ſurface, and afforded a very agreeable ſight by the reflection of the light from their ſmooth ſuperficies.

Of the Difference in the height of a Human Body, *between Morning and Night; by Mr.* Waſſe. Phil. Tranſ. N° 383. p. 87.

MR. *Waſſe*, having meaſured a great many ſedentary people and day-labourers of all ages and ſhapes, found the difference of their height in the morning and at night to be near an inch. He tried himſelf, when ſitting, and found it in the like manner; particularly, *Auguſt* 21. 1728, weather warm, no wind, he ſat down at 11 o'clock in the morning, and fixed an iron-pin, ſo as to touch it, and that but barely; afterwards he fatigued himſelf for $\frac{1}{2}$ an hour with a garden-roller; and the conſequences was, that at 12^h 30′ he could not reach the nail ſitting, by about $\frac{5}{10}$ of an inch: At 2 o'clock the ſame day he wanted near $\frac{6}{10}$ of an inch. On the 21ſt at 6^h 30′ in the morning, he touched the nail fully; and after the abovementioned exerciſe for only $\frac{1}{4}$ of an hour, at 7^h 14′ he fell ſhort, almoſt as much as before. On the 27. having ſat up late, he was faint and felt himſelf heavy upon the ground, and without any ſpring, and at 9 that morning he did not reach the nail, tho' he had uſed no exerciſe; he rid out, but he could not reach it that day. On the 28. he rid about 4 miles; and whereas at 6 that morning he reached the nail, by 8 he had loſt $\frac{6}{10}$ of an inch. *September* 19. he came ſomewhat tired from *Oxford*, and next morning at 8 he wanted $\frac{1}{2}$ an inch. If he ſtudied cloſely, tho' he never ſtirr'd from his writing deſk; yet in five or ſix hours he would loſe near an inch. All the difference he found between labourers and ſedentary people is, that the former are longer in loſing their morning height, and ſink rather leſs in the whole than

the latter. He could not perceive, that when the height is loſt, it can be regained by any reſt that day, or by the uſe of the cold bath. *April* 30. 1724. Mr. *Waſſe* meaſured his mare (who was 7 years old, ſtrong, ſhort back'd and well legged) before and after riding 20 miles, and could not perceive the leaſt difference in her height. The alteration in the human ſtature, he ſuppoſes, proceeds from the yielding of the cartilages between the *vertebræ* to the weight of the body in an erect poſture.

This curious obſervation was confirmed by ſeveral members of the *Royal Society*, and others who purpoſely tried the experiment.

Remarks on the foregoing Obſervation; *by Mr.* Becket. Phil. Tranſ. N° 383. p. 89.

MR. *Becket* obſerved farther, that the alteration has been more conſiderable in young perſons than in thoſe that have been aged. The trials, equally ſucceeding in a ſitting as in a ſtanding poſture, will naturally lead us to believe, that it muſt neceſſarily be from the trunk of the body, or ſome of its parts, that this remarkable alteration is brought about. Now every body knows, that the ſtandard of a perſon's ſtature has always been looked upon to be determined by the whole *compages* of the bones, adjuſted by the divine architect, according to the ſtricteſt rules of geometry: But there is ſomething ſo ſurpriſing in the ſtructure and diſpoſition of the ſpine, that nothing but ſuch a peculiar contrivance could ſo curiouſly have fitted it for the reſpective uſes and purpoſes it was ordained for. The thickneſs and ſhortneſs of the bones, with the intervening cartilages, aſſiſted by the bony proceſſes, diſpoſe it to a motion peculiar to itſelf: Whereas, had the bones been of any conſiderable length, upon bending the body, the articulations muſt have formed a large angle upon their innermoſt edges, and the ſpinal marrow continually liable to be injured; or had the cartilages been entirely wanting, it would have been as uſeleſs, as if it were but one bone; whereby being rendered incapable of bending the trunk of the body, it muſt have always remained in an erect poſture. But by the preſent diſpoſition of its parts, it is not only abſolutely ſecured againſt any ſuch inconveniences; but ſo ſmall a pillar is capable of ſupporting without hazard ſuch prodigious weights, as we are not wanting in our accounts of: Another particular which beſpeaks the utmoſt wiſdom and deſign in the contrivance of this part is the remarkable difference in the

the thickneſs of the cartilages placed between the bones of the ſpine; the *vertebræ* of the breaſt requiring but little motion, the cartilages there are but thin, in compariſon of thoſe of the loins, which being very thick, the loweſt more eſpecially, the motion is there vaſtly greater; and the cartilages being abundantly thicker before than behind, this is the reaſon that we bend our bodies ſo much more forwards than backwards: And by this admirable method of diſpoſing the thicker parts of the cartilages forwards, it is, that in all violent exerciſes, the parts contained in the belly and breaſt are in a great meaſure ſecured from any damages to which they might have been liable; becauſe by the pliableneſs and elaſticity of theſe cartilages, they break the violent ſhocks the *viſcera* muſt otherwiſe have neceſſarily ſuſtained upon ſuch occaſions. From what Mr. *Becket* has here remarked, in relation to theſe peculiar properties of the cartilages, placed between the bones of the ſpine, we may reaſonably ſuppoſe them to be certain compreſſible, dilatable, elaſtic bodies, which, like other bodies, endued with the ſame qualities, will naturally yield to any incumbent weight, ſufficient to force the particles of matter of which they conſiſt, into a more cloſe union; and that when this compreſſive force is removed, they will of themſelves recover their former ſtate: Now it is particularly to be obſerved, that the loweſt of all the cartilages of the loins is the thickeſt; and ſo conſequently, that it contains a greater quantity of matter than any of the reſt: By which means it becomes more diſpoſed to have its thickneſs diminiſhed, and that all of them gradually become thinner, even to the top of the ſpine. Now all ſuperior bodies if they come to an immediate contact, preſſing upon their inferior, it muſt neceſſarily follow, that the whole weight of the body, excepting the lower limbs, muſt preſs upon and be ſuſtained by the loweſt *vertebræ* and their cartilages: But theſe cartilages, as has been obſerved, being much thicker in this part than the other, and the incumbent weight bearing harder upon them, they muſt be unavoidably compreſſed more than the other; and ſo conſequently, when this weight is removed, their expanſion, by reaſon of their natural elaſticity, will alſo be greater. This being the natural ſtate and diſpoſition of theſe parts, during the whole ſpace of time we are uſually employed about our neceſſary avocations, till we diſpoſe ourſelves to reſt, the cartilages of the ſpine will, by their compreſſible and yielding properties, become more cloſe and compact from the perpendicular preſſure they ſuſtain; and ſo conſequently the ſpine, the only ſupport of

the trunk of the body, will become ſhorter. But when this ſuperior weight ſhall be entirely removed, by placing the body in an horizontal poſture, as it always is, when we are in bed, the compreſſed cartilages will, by their natural elaſtic power, begin gradually to enlarge themſelves, till they recover the ſame expanded ſtate they enjoyed, before they were forced to yield to the incumbent preſſure; and ſo conſequently, it will produce a conſiderable alteration in a perſon's ſtature, agreeable to the determined times mentioned in the preceeding *Tranſaction*. For, if we only conſider, that the aforeſaid compreſſive force will leſſen the thickneſs of all the cartilages, in proportion to the quantity of matter they contain; and that they are uſually about 24 in number, it will be no difficult matter to apprehend, that their natural expanſion being recovered by reſt, the aggregate of the whole of the expanſions may amount to about an inch. Now if the alteration be ſo conſiderable, occaſioned only by the bare incumbent weight of the ſuperior parts of the body, without any additional force applied to compreſs the cartilages ſtill cloſer; how much more may we reaſonably imagine it would be were the experiment tried on ſuch perſons, whoſe uſual employment it is to carry heavy burdens. Mr. *Becket* only obſerves this one particular, which is, that this alteration is not to be expected to be the ſame in aged perſons, as in thoſe that are younger; becauſe, the cartilages, as we advance in years, gradually grow harder, till many of them arrive to the ſolidity of a bone, that is, by degrees they loſe their ſpring or expanſive power, and at length continue in a compreſſed ſtate of reſt. And this is, undoubtedly, one principal cauſe, why old people not only ſeem to have loſt ſomewhat of their height, but are actually ſhorter.

The Variation of the horizontal Needle *at* London *in the latter end of the Year* 1722 *and the beginning of* 1723, *by Mr.* George Graham. Phil. Tranſ. N° 383. p. 96.

THE figure of the three needles, with which the experiments were made, was priſmatic; their length 12, inches nearly; their extremities, which pointed to the diviſions being filed to an edge, which made a fine line, perpendicular to the horizon. The caps of two were of cryſtal, the other of glaſs; they were well poliſhed on the inſide, in that part which touched the pin they moved upon. The box was braſs, and of a breadth ſufficient to admit 20 degrees on each ſide the middle line, and covered with a piece of ground glaſs; the circular arch

arches at the ends were raiſed ſo much above the bottom of the box, as to have their upper ſurfaces, upon which the diviſions were cut, lie in the ſame plane with the needle, and at ſuch a diſtance from each other, that the needle might play freely between them. A few of the degrees at the north end were divided into ſix equal parts, each diviſion being 10 minutes. It was eaſy, by the help of a convex glaſs, to determine the pointing of the needle to leſs than a quarter of theſe diviſions, or to about two minutes of a degree. The pin, upon which the needle moved, was of ſteel hardened and ground to a fine point; and by a ſpring placed in the box, the needle might be raiſed from off the point, and let down again at pleaſure, without removing the glaſs, or diſturbing the box. By this means both the ſharpneſs of the point and poliſh of the cap, were better preſerved from injury, when there was occaſion to move the box. A ſmall piece of braſs was made to ſlide upon that end of the needle, which pointed to the ſouth, for readily bringing it to an horizontal poſition; for, according to the different ſtrength of the touch, the north end of the needle will dip more or leſs. The bottom plate of braſs was a little broader and longer than the box; and its edges made lines, exactly parallel to the middle line of the diviſions; and for the greater ſecurity of placing the box in a right ſituation, there was a braſs ruler 30 inches long, having its edges even and parallel, excepting part of that edge, which was applied to the ſide of the box, that was a little filed off on the middle, that the ſide of the box, near its ends only, might touch the ruler. By this contrivance the two points of contact were as far aſunder, as the length of the box would admit of, and the other edge of the ruler making a longer line than the ſide of the box, afforded a better direction for giving it the ſame ſituation.

For determining the quantity of the variation, Mr. *Graham* got a meridian line, ſtretched upon the top of the houſe, between the rails of the leads, which were upwards of 15 foot aſunder, and the line was a little more than 39 inches above the leads. As this line was faſtened to two pieces of braſs, that were fixed in the rails, and was upwards of 15 foot long, no ſenſible error could ariſe in putting it up at any time. The compaſs box was placed upon a wooden ſtool, with three feet, that had nothing of iron about it, and its top ſet level by a plumb-rule: But finding, that in the open air the wind gave ſome diſturbance, he put up another line, after the ſame manner, in a room two pair of ſtairs high; this line was about the ſame length

length with the other, and 39 inches above the floor. Some time after he put up a third line, of the same length, in the room over this. By the method made use of in fixing these lines, they could not differ above 2′ of a degree from the meridian, or from each other. Before he had made any trials, he imagined no other difference would arise than what might be occasioned by the friction of the needle upon the point it was to move on, and having found that considerable in all the needles he had taken notice of, he took more than ordinary care to provide against it, and succeeded beyond his expectation. For, he several times observed all the three needles return so exactly to the same place, that he could not perceive the least difference; as likewise all three agree very nearly about the same time, when they have been placed in the same box immediately one after another, the box remaining unmoved. The first needle he made, was a little above three tenths of an inch broad, about ,06 in thickness, and weighed about an ounce *Troy*, together with the cap of crystal. After some trials with this needle, it was made narrower, not to exceed half a tenth of an inch, and it then weighed 5 penny weight and 5 grains. The second needle was at first about 3 tenths of an inch broad, and ,04 thick, together with the cap of glass; and after several trials it was made so much narrower, that its breadth was somewhat less than its thickness, and it weighed 2 penny weight and 5 grains. The third was nearly of the same dimensions with the second and weighed 2 penny weight and 3 grains. When the two first needles were made narrower, care was taken, that the files, made use of for filing the north ends, did not touch the south ends; and after they were made lighter, he tried them both, before they were fresh touched upon the stone, and found no sensible difference in their direction. The reason of making the two first needles so heavy was to try, whether they would return more constantly to the same situation than lighter ones. But notwithstanding each of them would settle very exactly in the same place, for a great number of trials made immediately after each other; yet he found them at different times to differ considerably from their former directions.

This occasioned his making them narrower, fearing their breadth had some way contributed to their irregularity. But after the alteration, he found the same thing happened, tho' he could find nothing of it to proceed from any friction upon the point. This made him prefer the lighter needles, as less apt to injure the point they moved upon, and as exact in returning to the

the ſame ſituation. After ſeveral trials, he found all the needles he made uſe of, would not only vary in their direction on different days; but frequently at different times of the ſame day; and this difference would ſometimes amount to upwards of $\frac{1}{3}$ a degree in the ſame day, ſometimes in a few hours. And this alteration he obſerved, whether the needles were drawn aſide immediately before the obſervation, or ſuffered to remain undiſturbed. For, he has left the box ſtanding for ſeveral days together, without ever diſturbing the needle, only he has taken notice what it pointed at, and the time of the day, and he could ſometimes perceive a very ſenſible alteration in a few minutes. But whether it ſtood near its greateſt or leaſt variation, or whether he drew the needle to one ſide with a key a few degrees or a greater number, it would conſtantly return to the ſame place it ſtood at immediately before. Sometimes he took the needle out of the box and put it in again, and this he ſeveral times repeated in an hour. At other times he took down the box from off the ſtool, and put it up again; but found no alteration in its direction: So that he found it of no conſequence, whether the needle was drawn aſide or let alone, the ſhaking of the floor by walking upon it, or the trembling of the houſe by the coaches in the ſtreet, was ſufficient to overcome the ſmall friction upon the point. When Mr. *Graham* made the obſervations, he was very careful to have no keys nor iron about him, that could affect the needle.

The box was placed in the room upwards of ſix foot from the neareſt wall, and upwards of 13 foot from the grate in the chimney, and no iron could at any time be brought near it without his knowledge. Yet after all, he was not ſatisfied, that it was out of the reach of iron, and that the variation ſhewn by it is the true quantity; but he was very ſure there was no change of circumſtances in the room, that could affect it; for, if there were any ſuch materials in the wall, or floor, their diſtances and ſituations continued the ſame. But for a farther confirmation of this irregularity, he put one of the needles into a wooden box, divided with a few degrees, as the other, and placed it at the ſame meridian line, at the diſtance of three foot and $\frac{1}{2}$ from the other, and found both needles nearly agreed in their alterations. The needles were all touched by that excellent loadſtone, preſented to the *Royal Society* by the Lord *Paiſley*. It is to be taken notice, that the needles were not touched upon the naked ſtone, but with its armour on, generally upon that part of the capping neareſt the poles; but he could find no difference in

the

the direction, by touching upon another part: Farther, when he obſerved the needle increaſing or decreaſing in its variation, he has very frequently, with a key, drawn it the contrary way ſeveral degrees, and then letting it return very gently, till it has been within a degree or leſs of the place it ſtood at immediately before, he ſtopped it there for ſome time, by holding the key at a proper diſtance; and withdrawing his hand gradually, tried to make it ſtand ſhort of its former place, but could never ſucceed. By this and ſeveral other methods made uſe of, he was well aſſured theſe changes in the direction are owing to ſome other cauſe than the friction of the needle upon the pin; but what that cauſe is he could not tell: For, it ſeems to depend, neither on heat or cold, a dry or moiſt air, clear or cloudy, windy or calm weather; nor on the height of the barometer. The only thing that has any appearance of regularity is, that the variation has been generally greateſt, for the ſame day, between the hours of 12 and 4 in the afternoon; and the leaſt about 6 or 7 in the evening.

March 8. 1722. A piece of braſs was fixed to a wooden box; and a few degrees were divided into 10′ each, as in the braſs box, to try, if both needles would be alike affected in the ſeveral alterations. This wooden box was placed at the ſame meridian line, and about the diſtance of three foot and ½ from the other.

	Braſs box. Needle 2 = 5.		Needle 2 = 5
	° ′	h. ′	° ′
March 8.	14 = 30 —	3 = 00	14 = 25 +
	14 = 20	3 = 15	14 = 20
	14 = 15 +	4 = 00	14 = 10
	14 = 20	4 = 15	14 = 15
	14 = 25	5 = 00	14 = 20
	14 = 25	5 = 30	14 = 20
	14 = 15	5 = 45	14 = 10
	14 = 00	5 = 57	14 —
	14 —	6 = 8	13 = 55
	13 = 50	6 = 15	13 = 40
	14 = 20	6 = 38	14 = 15 +
	14 +	6 = 48	14 = 00
	14 = 00	6 = 54	14 —
	14 = 5	7 = 5	14 +
	14 = 10	7 = 15	14 = 5
	14 +	12 = 00	14 +

Braſs

Brass box. Needle 2 = 5.		Needle 5 = 5.
March 9. ° ′	h. ′	° ′
14 = 10	9 = 30	14 = 10
14 = 10 +	10 = 00	14 = 10 +
14 = 10	10 = 15	14 = 10
14 = 10 +	10 = 30	14 = 10 +
14 = 15	11 = 00	14 = 15 —
14 = 00	8 = 15	14 = 00
14 = 00	11 = 50	14 = 00

March 10. ° ′	h. ′	° ′
14 = 10 +	10 = 00	14 = 10
14 = 15	11 = 00	14 = 10 +
14 = 15	12 = 00	14 = 10 +
14 = 15 +	12 = 45	14 = 10 +
14 = 15 +	1 = 00	14 = 10 +
14 = 15 +	1 = 30	14 = 10 +
14 = 15 +	1 = 45	14 = 10 +
14 = 15 +	2 = 00	14 = 10 +
14 = 15	3 = 30	14 = 10
14 = 15 +	4 = 00	14 = 10 +
14 = 15 —	5 = 30	14 = 10 —
14 = 10	6 = 00	14 = 5
14 = 00	6 = 15	14 = 00
14 —	6 = 30	14 —
14 +	7 = 30	14 +
14 = 5	7 = 45	14 +
14 +	12 = 00	14 +

March 30.

The needle 2 = 5 which was in the brass box, was this day put into the wooden box, and a new needle put into the brass box, weight 2 = 3.

No remarkable change happened to either needle till *April* the 5.

Needle 2 = 3 in the braſs box.			Needle 2 = 5.
	° ′	h. ′	° ′
April 5.	14 = 5	9 = 00	14 + 00
	14 = 10	1 = 30	14 = 5
	14 = 10 —	5 = 30	14 +
	14 —	8 = 15	14 —
	13 = 50 +	8 = 37	13 = 45
	13 = 55 +	9 = 45	13 = 45 +
	14 —	10 = 25	13 = 50
	14 = 00	10 = 45	13 = 55
	14 +	11 = 00	14 = 00

The firſt column ſhews the variation of the needle in the braſs box. The third the variation of that in the wooden box. The ſecond column ſhews the time, by the clock, when the obſervations were made.

April 15. 1723.		*April* 16.	
° ′	h. ′	° ′	h. ′
14 = 30 —	9 = 00	14 = 30 —	9 = 30
14 = 30 —	10 = 00	14 = 30	11 = 00
14 = 30	11 = 30	14 = 30	12 = 00
14 = 30 +	12 = 30	14 = 30 +	1 = 10
14 = 30 +	1 = 30	14 = 30 +	1 = 40
14 = 30	3 = 30	14 = 30	2 = 45
14 = 30	4 = 10	14 = 30	5 = 00
14 = 30 —	5 = 30	14 = 30 —	6 = 00
14 = 20	6 = 18	14 = 25	6 = 20
14 +	7 = 8	14 = 20	6 = 30
14 = 00	7 = 50	14 = 15	6 = 35
14 = 00	8 = 15	14 = 10	6 = 40
14 = 15 +	8 = 20	14 = 10 —	6 = 45
14 = 15 +	8 = 40	14 = 5	6 = 49
14 = 15 +	12 = 15	14 = 00	6 = 57

14 =

° ′	h. ′
14 = 00	12 = 27
14 = 00	12 = 32
14 = 00	12 = 35
14 = 00	12 = 43

Wind at S. W.

14 = 15 +	8 = 20
14 = 20 −	8 = 30
14 = 25 +	9 = 00
14 = 25	12 = 12
14 = 25	12 = 21

Day warm, cloudy in the morning, evening clear.

April 19.

° ′	h. ′
14 = 30 −	8 = 35
14 = 30 −	9 = 00
14 = 30 −	1 = 30
14 = 30 −	2 = 00
14 = 30	3 = 30
14 = 30	4 = 00
14 = 20	5 = 00
14 = 25	5 = 38
14 = 25 +	5 = 45
14 = 30 −	6 = 00
14 = 30 −	6 = 45
14 = 25	7 = 00
14 = 20 −	8 = 00
14 = 20	9 = 00
14 = 20 +	10 = 00
14 = 25	11 = 00
14 = 25	11 = 15

° ′	h. ′
14 = 00	7 = 10
14 +	7 = 20
14 = 5 +	7 = 30
14 = 10	7 = 45
14 = 15	8 = 00
14 = 35 +	1 = 52
14 = 40 +	2 = 30
14 = 20	3 = 30
14 = 25	3 = 45
14 = 30 −	4 = 00
14 = 25 +	6 = 45
14 = 20	7 = 00
14 = 30 −	7 = 35
14 = 20 +	12 = 50

Day cold, wind at east.

May 3.

° ′	h. ′
14 = 10	9 = 30
14 = 15	11 = 10
14 = 15 +	12 = 40
14 = 15 +	2 = 20
14 = 10 −	5 = 20
14 = 10	6 = 5
14 = 10	6 = 45
14 = 10 −	7 = 5
14 = 5	7 = 15
14 +	7 = 30
14 = 00	7 = 42
14 +	8 = 00
14 +	9 = 38
14 +	10 = 15
14 +	11 = 00

Day warm, wind at east, some thunder in the afternoon.

May 2.

14°	= 25′	9h	= 30′
14	= 30 +	10	= 30
14	= 35 —	11	= 30
14	= 35 +	1	= 00
14	= 10	4	= 55
14	= 10	6	= 00
14	= 00	8	= 15
14 + 1		1	= 00

Wind at east.

May 5.

14°	= 10′ +	9h	= 30′
14	= 15	10	= 45

Day cold, wind easterly

May 4.

14°	= 5′	9h	= 15′
14	= 5	9	= 30
14	= 10 +	1	= 35
14	= 10 +	3	= 17
14	= 10 +	3	= 50
14	= 15 +	12	= 30
14	= 20	1	= 57
14	= 20	2	= 45
14	= 20	3	= 25
14	= 20 +	4	= 35
14	= 15 +	5	= 30
14	= 15 +	6	= 10
14	= 15 —	12	= 7

Day clear, wind at east.

All these observations were made with the lightest of the three needles, the compass-box remaining unmov'd the whole time. From *February* 6. 1722. to the 10th of *May* following, Mr. *Graham* made upwards of 1000 observations in the same place; and the greatest variation westward was 14°=45′ and the least — — 13°=50′. It was seldom less than 14°, or greater than 14°=35′

Observations on Dr. Eaton's Stiptic; *by Dr.* Sprengell. Phil. Trans. N° 383. p. 108.

THE method of curing fresh wounds in a few days, without suppuration, where neither nerves, large vessels, bones, or any of the *viscera* are concerned, has been long ago observ'd: *Purman*, a famous surgeon of *Breslaw*, in his *Chirurgia curiosa* tells us of a mountebank, who gave himself 13 wounds, by incision, in the upper part of his left arm; and thereupon applied his *nostrum*, and with the help of a good roller, he was cur'd in two days time.

Next

Next he mentions a martial ſtiptic, which ſtopp'd bleeding incomparably well, and heal'd freſh wounds (as he affirms) in two days; eſpecially if the patient took withal a few drops inwardly. This is likewiſe mentioned by *Blegny* near 30 years before.

When Dr. *Sprengell* was in *France*, he found that ſeveral trials were there alſo made, with a ſtiptic ball, mix'd with *French* brandy, by ſtriking a cock thro' the head, opening the crural artery of a dog, or chopping off a dog's leg, *&c.* But he found it did not amount to any thing of conſequence; yet he had ſtill a ball, made upwards of 20 years before, of filings of iron and an equal quantity of tartar, mix'd with *French* brandy upon a marble: This, with ſome alteration, was afterwards publiſh'd by the famous *Helvetius*, phyſician to *Lewis* XIV. of *France*, in a book, entitl'd, *Recueil des methodes pour la gueriſon des diverſes maladies*, which was reprinted in *Holland* in the year 1710. This preparation was then ſold by *Pierre Rottermond*, apothecary at the *Hague* in *Holland*.

The recipe for his medicinal ball is as follows. Take four pounds of the filings of ſteel, and eight pounds of tartar, well powdered; mix theſe well together. and put them in a new earthen pot, and pour thereon as much *French* brandy, as will make it into a poulteſs; let this ſtand fermenting in a cellar for four days, and ſtir it between whiles; then put it into *balneo mariæ*, and diſtill it *ſ. a.* with a moderate fire, to draw off the brandy: When you find, that nothing but the phlegm comes over, remove it from the fire and take out the maſs, ſtamp it very fine, that there may not remain the leaſt lump; then mix it again as before with a ſufficient quantity of brandy, and put it into the cellar to ferment, as before, and then diſtill it a ſecond time. This operation may be repeated ſeven or eight times: But the laſt time mix your maſs well upon a marble, and make it into two ounce balls. One of theſe balls is ſteep'd in a pint of good *French* brandy, a little warm'd, and hung only in it by a wire, till the brandy have receiv'd the colour of the ball: But if you are in haſte, then grate a ſufficient quantity of the ball in ſome brandy, ſtir it well, and you may uſe it that very inſtant.

The author, undoubtedly, thought by often grinding, fermenting and diſtilling this maſs, to comminute and ſubtilize its particles, ſo as to make it more fit to contract the fibres and veſſels of a wound, and to prevent ſtagnations of the fluids, both within and without, upon contuſions; but the ſucceſs

ſucceſs did not anſwer, and therefore it was laid aſide. Neither did *Helvetius* ever recommend it as an univerſal ſtiptic, aſtringent or conſolditating medicine; but merely in freſh wounds, and that only for a firſt dreſſing, and where people liv'd at a diſtance, and could not get immediate aſſiſtance from a ſurgeon. Beſides, he makes ſeveral exceptions, where it ſhould not be us'd; and in general, adviſes where chalybeate medicines may be made uſe of according to experience.

But as a balſamic ſtiptic was publiſh'd by one Dr. *Eaton*, for ſtopping all manner of bleeding, either external or internal, and that without any manner of exceptions; this made Dr. *Sprengell* deſirous to ſee it, and ſoon after he had an opportunity of examining it: He immediately found, that this was the ſame old medicine, which was alſo got into *England*, after other countries had diſcarded it. But he neglected it at that time, as not worth his notice, till he ſaw a treatiſe of conſumptions, publiſh'd by Sir *Richard Blackmore*, giving it the greateſt encomium, that ever was given to any invention whatever. 'For, *ſays he*, Dr. *Eaton*'s balſamic 'ſtiptic bids fair for the credit of a certain remedy in ſtop-'ping of blood outwardly or inwardly, where the *craſis* of the 'blood is not entirely ruin'd, and will be of more ſervice to 'mankind, than all the diſcoveries made by *Galenical* com-'pounders of drugs and ſyſtematical methodiſts.'

Dr. *Sprengell*, finding this remedy recommended in ſo extraordinary a manner, by ſo eminent a phyſician, began now to think, tkat poſſibly he might have been miſtaken, and therefore deſir'd Mr. *Winterbottom*, an apothecary in *Bowlane*, immediately to prepare the recipe, as preſcrib'd by Dr. *Helvetius*. When this was ready, he ſent for a little of Dr. *Eaton*'s ſtiptic, and tried them both with galls before ſeveral gentlemen; when the tincture was the ſame, *viz.* a deep purple. He then precipitated the contents with old hock, and found the precipitated matter to be the ſame in both. Dr. *Sprengell*, not contented with his own enquiry, ſent ſeveral ſmall quantities to others, and went himſelf to Mr. *Godfrey*, the chemiſt: And they all told him, there was no difference. The Dr. tried ſeveral ways to find out its balſamic quality (whence it has its name) but found none. Upon which he was ſurpriz'd, that a man who had a mind to vend a thing, as a ſecret, had not done ſo much as to alter it, either in taſte, ſmell, or colour; and yet this might very well

well have been done, without robbing it of its virtue in the leaſt.

The Dr. next tried theſe two upon the crural artery. Having procur'd a good middle ſiz'd dog, Mr. *Kanby* laid the artery bare, and open'd it with a lancet lengthways, for near half an inch. The old method was cutting the artery a-croſs; and then there was no neceſſity of any ſtiptic at all, nor indeed here neither. But at firſt *Helvetius*'s tincture was applied, which ſtopp'd the bleeding; then opening the artery again, Dr. *Eaton*'s was tried with the ſame ſucceſs: Afterwards the Dr. had the artery open'd in the other thigh, and trying it only with *French* brandy, he found this did as the other two. He opened the artery again, and diſſolving a little *ſal martis* and *ſaccharum ſaturni* in *French* brandy, he applied that, which anſwer'd in the ſame manner. This made the Dr. immediately conjecture, that there was but little virtue in either of them; but only that the brandy, by its great heat, did merely contract the fibres of the artery, which, no doubt, might be ſomewhat aſſiſted by the ſteel; but this could not be much. The Dr. then reflected on the ſmallneſs of the crural artery in a dog, and not to be compar'd with that of a man, and that a little pledget of lint might have ſtopp'd the blood, without more to do, as well as the temporal artery, when opened with a lancet, which was done, and the pledget of lint ſtopp'd it accordingly. Thus far as to its external uſe. If only, according to *Helvetius*, it had been ordered to be taken internally, in freſh wounds and contuſions, this might have been ſufferable: But when, without exception, Dr. *Eaton*, as alſo Sir *Richard Blackmore*, recommended it in all outward bleedings, Dr. *Sprengell* thought proper to make ſome animadverſions thereon. For, Sir *Richard* himſelf ſays, in his *Treatiſe of Conſumptions*, p. 99. 101. that in ſpitting of blood, there is an orgaſm, or ſtimulating ferment; What is this but a feveriſh indiſpoſition? And is there any hemorrhage without it? Now if ſo, will not brandy and chalybeats heighten it? Which, by their heat and *ſtimulus*, brace and irritate the fibres, and accelerate the motion of the blood. And will not the blood then take up more room, and preſs harder againſt the ſides of the veſſels, and whatever oppoſes it? Is not this the way to cauſe an orgaſm and hemorrhage?

Dr. *Eaton* himſelf tells us in his book, p. 57. That it did very much over-heat a gentlewoman, and that her bleeding

ing ſtill continu'd after the taking it, and ſhe might have periſh'd, had not a ſurgeon given her a cooling and aſtringent apozeme. And but juſt before, p. 47. he complains of a phyſician that was not willing his patient ſhould take it, who had a hectic fever upon her; becauſe he was afraid it was too hot.

Since the former trials, the Dr. deſired Mr. *Ranby* to open the carotid artery of a dog, thinking that this artery might give him more ſatisfaction than the crural arteries had done; to try the ſtiptic qualities of *Helvetius* and *Eaton*'s tinctures. Having laid bare the jugular vein, divided it and tied it, that its bleeding might not hinder the finding the carotid artery: Upon opening the artery with a lancet, the blood ſpouted forth, and applying *Helvetius*'s tincture, the bleeding ſtopp'd. He took it off in leſs than a minute, and made it bleed again, but it bled but little, and then he applied Dr. *Eaton*'s ſtiptic: He fill'd up the wound with lint, ſtitch'd up the *cutis*, then untied the dog and let him looſe; when, ſome time after, the Dr. ſeeing him, found he had bled a good deal, and was ſtill bleeding. The Dr. was very well ſatisfied, that the artery being ſo very ſmall, he would not bleed to death, and if he had nothing but lint upon it, it would have done as well. It is to be obſerv'd, that the carotid arteries are largeſt in proportion in human bodies, and that this artery of the dog was but a ſmall matter bigger than the crural arteries of the former dog. This ſhews, that the ſtiptic quality of theſe tinctures is very inconſiderable; and that *Helvetius*'s tincture is rather better than Dr. *Eaton*'s, if there is any difference at all; tho' that, he believes, was owing to the brandy; for, Dr. *Sprengell*'s brandy was ſtronger than Dr. *Eaton*'s.

The Dr. enquired for the dog next morning, and found him alive and well, only hanging his head on one ſide, which was owing to the muſcles being cut thro'.

The Specific Gravities of ſome Bodies; *by M.* Fahrenheit. Phil. Tranſ. N° 385. p. 114. *Tranſlated from the* Latin.

Gold	19081
Mercury	13575*
Lead	11350
Silver	10481
Swediſh copper	8834
Japoneſe copper	8799
Iron	7817

Malacca

Malacca tin	7364
Engliſh tin	7313
White marcaſite	9850
Regulus of antimony	6622
Braſs	8412
Rock-chryſtal	2669
Homogeneous pyrites	2584
Potaſh depurated from the filth, and a neutral ſalt, in which it more or leſs abounds	3112
The above neutral ſalt	2642
Sea ſalt	2125
Nitre	2150
Alum	1738
Very white ſugar	1608½
Oil of vitriol	1877½*
Lixivium of potaſh, impregnated with as much ſalt as was poſſible	1563 *
The ſame prepar'd at another time	1571⅓*
The better ſort of *aqua fortis*	1409*
Spirit of nitre	1293½*
Rain water	1000*
Rape-ſeed oil	913
Alcohol of wine	826
The ſame more dephlegmated	825

M. *Fahrenheit* made the experiments ſeveral ways; for, he weigh'd fix'd bodies, as is commonly done, firſt in a nice pair of ſcales in air, and then in rain water: He diſcover'd the weight of ſalts firſt in air, and then in a proper liquor, after which he compar'd it with the gravity of water; he diſcovered the gravities of the liquors, ſometimes by the areometer, deſcrib'd in a following *Tranſaction*, and ſometimes in veſſels, as here delineated.

A concave glaſs ball (as repreſented A Fig. 7. Plate XI.) blown pretty large at the flame of a lamp, furniſh'd with two ſmall glaſs tubes oppoſite to each other, as BB; the extremities of which were open, tapering, and ſomewhat incurvated, that the liquor might not run out. Beſides, the ball was made ſomewhat flat in its lower part, that it might ſit the better on the ſcale.

The bolt-head A Fig. 8. blown of very thin glaſs at the flame of a lamp, had a pretty large neck, whoſe aperture was ſhut with the lid B, made as accurately concave on the inſide as poſſible.

By means of this bolt-head, the ſpecific gravities of ſalts may be found in the following manner. In the firſt place the bolt-head is fill'd with a proper liquor (in which the ſalt, whoſe gravity is ſought, will not diſſolve) and when the weight of the liquor is known, it is pour'd out, and the veſſel well dried: After this it is almoſt fill'd with the ſalt, whoſe weight is ſought; upon knowing which, the interſtices of the ſalt are fill'd with the liquor, and the increaſe of weight by the additional liquor, is ſought: if this increaſe of weight be ſubſtracted from the whole weight of the liquor, the remainder will give the gravity of the liquor excluded by the ſalt.

The neutral ſalt of potaſh cauſes no ebullition in ſpirit of nitre, it precipitates mercury, diſſolv'd in ſpirit of nitre, of a white colour; and when laid upon live coals, it decripitates and flies into ſmall pieces.

The nitre was melted in a crucible over the fire, that it might be freed of all its moiſture; and that the interſtices, filled with air, might be fill'd with the nitre itſelf.

The gravities of the liquors, mark'd with an aſteriſm, are reduced by calculation to the 48th degree of M. *Fahrenheit*'s thermometers; and ſome of them were mentioned in the experiments on the degrees of heat in boiling, in *Phil. Tranſ.* N° 381.

The moſt ſimple method of finding the difference of gravity, which ariſes from the different temperament of fluids, is, firſt to fill ſome veſſels with a liquor not very hot (but whoſe degree of heat ſhould be diſcoverable by the thermometer) and then to weigh it; and afterwards to fill the ſaid veſſel again with hotter liquor, and weigh it as before. If the degree of heat in this ſecond experiment be again mark'd, you have the difference of the gravity of the liquor, caus'd by an intermediate degree of heat, which may then be eaſily calculated for every other degree.

The experiments were made in air; its gravity therefore is to be added to each number, in order to have the gravity of the liquors in *vacuo*. But the ſpecific gravity of air is to that of water, as 1 to 1000 nearly.

Some new Experiments concerning the different, and ſometimes contrary Motion of the Sap *in* Plants; *by Mr.* Fairchild. Phil. Tranſ. N° 384. p. 127.

MR. *Fairchild* ſhew'd ſome experiments before the Royal Society, which were allow'd to be new and uſeful; this made him try farther experiments, in order to ſhew the courſe of the ſap, which he found by experience ſo uſeful, that he could make barren trees fruitful, decaying trees healthful, and render the ſyſtem of gardening and planting more ſerviceable to the public.

He ſhew'd the *laureola*, grafted upon the *mezereon*, and the *ever-green* oak of *Virginia* upon the common *Engliſh* oak; both which hold their leaves all the winter, and are in a good ſtate, and flouriſhing, tho' grafted on plants that drop their leaves in winter; which plainly proves, that the juices riſe upwards, in winter, in thoſe plants that drop their leaves, otherwiſe the *ever-greens* that are grafted on them, would ſoon periſh.

Mr. *Fairchild* takes it, that by grafting the various foreign oaks upon the *Engliſh*, the timber might be made the more firm and laſting, than it is in its own nature, when rais'd from foreign acorns: For, as the crab-ſtock makes the wood of the apple-tree more firm and laſting than that on the apple-ſtock; and the peaches and almonds, budded on plumbs, are more laſting than thoſe on peach-ſtocks; ſo by the contrary rule, all firm timber, grafted on ſpongy ſtocks, would be made worſe than it would on its own bottom. For inſtance, if that which is call'd the *Engliſh* elm ſhould be grafted on that which is call'd the *Dutch*, it would partake a good deal of the ſpongy juices of the ſtock, whereby the timber would become unfit for the purpoſes it is now us'd for.

The firſt experiment Mr. *Fairchild* had to offer, was made on the *New England* cedar, or rather juniper, grafted on the *Virginia*; and what was remarkable in it was, that the branch which was grafted was left ſeveral inches below the grafting, which part continued growing, as well as the upper part above the grafting.

The ſecond was on the *viburnum*, whoſe top being planted in the ground, became roots; and the roots, being turn'd up, became branches. He found the plant in as good a ſtate of growing, as it was in its natural ſtate.

The third was on a pear-tree, which he enarch'd upon two pear-ſtocks in *March* 1721-22, and was then in a good flouriſhing ſtate, with a branch in bloſſom, and receiv'd no nouriſhment but by the two enarch'd branches, the root being out of the ground; and tho' it was done upwards of two years before, it was then ſhooting ſuckers out of the root, which proves that the branches are as uſeful to ſupport the roots, as the roots the branches; and it is, therefore, no wonder, that ſo many trees miſcarry in planting, when there are no branches left on the head.

The fourth was on the cedar of *Libanus*, grafted on the *laryx*, which drops its leaves in the winter; yet maintains the cedar in as flouriſhing a condition, as if it had been on a tree, that held the leaves all the winter; and the part of the graft left below the grafting was in as good health, as the part above it.

An extraordinary Cure by Sweating *in hot Turf; together with a Deſcription of the* Indian Hot-houſes; *by Mr.* Paul Dudley. Phil. Tranſ. N° 384. p. 129.

IN 1704 *Peter Coffin* Eſq; of *Exeter* in *New England*, being then 74 years of age, had taken a great ſurfeit as it was ſuppoſed, by drinking cold water in a very hot day, after having heated himſelf in the woods. This ſurfeit ſettled chiefly in his right ſide, but gave him a racking pain all over his body, and particularly deprived him of the uſe of his right arm. In this condition he kept his bed for nine weeks, and his recovery, conſidering his age, was deſpaired of; when his ſon (from whom Mr. *Dudley* had the account) propoſed ſweating him in turf. The father readily agreed to it, having uſed ſeveral medicines, without any effect. Immediately orders were given to cut a large oven full of turf, the pieces being about 18 inches ſquare. The turf itſelf was of *Engliſh* graſs, and only the ſwerd or top of the earth with the graſs. Before the turf was put into the oven, the Dr. rubbed the graſſy ſide of the turf with ſome ſpirit or oil (but of what ſort the Dr. would give Mr. *Dudley* no account) and then doubled the graſs-ſides together, and ſo ſet them in. When they were well baked, which was in about two hours, he took them out and made a bed of them upon the floor (the place for the head raiſed) as ſoon as that was done, he ordered his father to be taken out of his bed without his ſhirt but wrapped up in a ſheet, and laid upon the hot turf,

and

and then he proceeded to cover him over with the reſt of the turf, more eſpecially on his ſide, where the ſeat of his pain was, but they laid none upon his breaſt or head; then they covered him with blankets to keep the heat in. While the father was in this bath, the ſon gave him warm cordials, to prevent fainting, which he was in great danger of: After he had lain thus about $\frac{3}{4}$ of an hour, which was as long as he could bear it, he was put naked into the bed, very well warmed, where in a few minutes he fell aſleep, and ſweated to that degree, that it ran thro' his pillow and bed, upon the floor. After about two hours ſleep they dried him and put him on warm cloaths, and he found himſelf much eaſed and refreſhed: This was in the morning; and before night he walked about the houſe, his pain being in a manner all gone. The next day the Dr. repeated his cordials; and the fourth day he ſweated him a ſecond time, in the ſame manner as above; and next day he went abroad about his buſineſs, and lived 11 years after, in perfect health and free from pain. The Dr. told Mr. *Dudley*, great care muſt be taken, that the patient do not lye too long in the turf; and even $\frac{1}{4}$ of an hour may be ſufficient for ſome patients; and whenever the patient begins to fetch his breath ſhort, or to faint, he muſt be put to bed immediately; and the phyſician or operator muſt by no means omit his cordials.

Sweating houſes were common among the *Aborigines*, when the *Engliſh* firſt came into *New England*; tho' now but little uſed. A gentleman of the iſland of *Nantucket*, where the *Indians* ſometimes practiſe it, even at this day, or very lately, gave Mr. *Dudley* the following relation. The cave was uſually four foot high, and eight foot in diameter; the roof ſupported with ſticks or boards, covered with earth, and they dug it in the ſide of a hill, and as near as poſſible to ſome river, pond or place of water. The entrance into this cave was ſmall, and the door (when any perſon was ſweating) was covered with a blanket or ſkin; near the cave they made a good large fire, and heated a parcel of ſtones, to the quantity of 500 weight, and rolled them in red hot, piling them up in the middle of the cave. When this is done, the *Indians* go in naked, and as many as pleaſe ſit round the heated ſtones; as ſoon as they begin to grow faint, which may be in $\frac{1}{4}$ of an hour, they come out and plunge themſelves all over in the water for a minute or two; and then in again, as long as they can well bear it; and ſo into the water a ſecond time and then they dreſs themſelves. This has been uſed with ſucceſs for colds, ſurfeits, *ſciatica's* and

pains

pains fixed in the limbs; and even the *Engliſh* have ſeveral times found relief by it.

Mr. *Dudley* did not underſtand, but that it might be practiſed at any time of the year, without hazard, or inconveniency. The *Indians* often uſed it both before, and after long journies, hunting or voyages, to ſtrengthen and refreſh themſelves.

An Attempt to account for the riſing and falling of ſome Ponds near the Sea, or ebbing and flowing Rivers, where the Water is loweſt in the Pond, at the time of high Water, and higheſt in the Pond, at the time of low Water in the Sea or River; as alſo for the increaſe or decreaſe of the Water of ſuch Pools and Brooks, as are higheſt in dry and loweſt in rainy Seaſons, with Experiments to illuſtrate the ſolution of the Phænomena; *by Dr.* Deſaguliers. Phil. Tranſ. N° 384. p. 132.

HERO Alexandrinus and other hydraulic writers, have deſcribed a cup (called a *Tantalus*, from its effect) which will hold any liquor very well, when it is not filled above a certain height, marked in the cup, but if it be filled higher, not only the liquor above the mark will run out, but the whole liquor contained in the cup. This is performed by a ſyphon in the cup, which is ſometimes concealed to make the effect the more ſurpriſing.

The cup A B (repreſented Fig. 9. Plate XI.) has a viſible ſyphon C E D therein; the cup (Fig. 10.) has the ſame, conceal'd by the figure of a man, repreſenting *Tantalus* in the fable; and the cup (Fig. 11.) has its ſyphon more concealed, as it is carried up into the handle. Any of thoſe cups will hold water very well, provided they are not filled up above the line F G; for, then not only the liquor above F G will run out; but all the liquor in the cup, as low as D, the orifice of the ſhort leg of the ſyphon.

Experim. 1. In the veſſel *a b c d* (repreſented Fig. 12.) is placed an open wooden box A B C D, filled with water as high as the line L M; another box or plug E F G H made tight and containing weights to ſink it, is made to let down into the water between the partition I K and the end A B of the abovementioned box; but when it is not to preſs the water up to I O (as it does when let down) it is drawn out of the water by the weight *m*, which pulls it up by the bar *i k*, faſtened to a lever, moving round the centre *l*.

When

When by means of the plug, the water in the ſpace A B K I is puſhed up to I O, by paſſing under K; it runs out thro' the ſpout P Q (whoſe paſſage is gaged by a little ſluce P *p*) and falls into the veſſel R S, made of an oblong figure like a fiſh-pond, and having a ſyphon at S, ſo as to make it a *Tantalus*, or in the nature of the cups abovementioned.

Let the weight *m* pull up the plug E F G H, and the water, having filled R S, will run down below the orifice P to M.

The *Tantalus* R S beginning to run out, as ſoon as full, will for the abovementioned reaſons, continue to run till it be all emptied; and as it diſcharges itſelf into another *Tantalus* T V (whoſe ſyphon is at V) this laſt *Tantalus* will alſo, when full, begin to run out, and its water go down to X Y *a*.

If the plug be let down gradually, as ſoon as the water begins to run out of the laſt *Tantalus* T V (and the firſt *Tantalus* R S be covered, ſo as to be concealed from ſight) it will appear to the ſpectators, that the cavity T V, repreſenting a pond near an ebbing and flowing river (as the Dr. was credibly informed there is ſuch an one at *Greenhith* in *Kent*, between *London* and *Graveſend*) always riſes, whilſt the water at N O (or the tide) falls to L M; and always ſinks, while the water at L M, or the tide, riſes to N O.

Exp. 2. Let the water in the box A B C D not be made uſe of; only the veſſel Z filled every half hour; it will empty itſelf in a $\frac{1}{4}$ of an hour, falling like rain and dropping thro' the leaden platform *ef* into the concealed *Tantalus* R S, which will not begin to run, till this artificial rain be over. Then in $\frac{1}{4}$ of an hour more, the *Tantalus* R S will have emptied itſelf into the viſible *Tantalus* T V, which will be filling all the time after Z has done running; or in the dry ſeaſon and as ſoon as T V is full, it will begin to run out thro' its ſyphon V, at the end of the half hour, when the veſſel Z or ſieve runs again; that is, at the return of the rainy ſeaſon.

This laſt experiment may be eaſily applied to thoſe ponds or brooks, that are high in dry weather, and low in wet weather; of which kind the Dr. was told there is a brook at *Lambourn* in *Berkſhire*.

If it be objected, that ſuch ponds are full for ſome time, which a *Tantalus* cannot be, becauſe it begins to run out as ſoon as full; that may be eaſily ſolved by ſuppoſing the hidden *Tantalus* (or intermediate cavity between the river and pond) to contain more water than the viſible one, provided it does not

contain

contain ſo much, as not to be emptied, before the return of the tide.

The ſame ſolution will ſerve for wet and dry ſeaſons, only ſuppoſing the cavities larger.

If it be aſked, where the water of the viſible *Tantalus*, near a river can run; it may be anſwered, that all this may happen, tho' the ſecond, or loweſt *Tantalus*, ſhould have its bottom higher than low water mark in the river; and as to the ſyphons, which are of a particular make in the cup; tho' ſuch be not ſuppoſed to be in the earth; yet any long paſſage, riſing in the middle, will anſwer the end. A B C D (Fig. 13.) repreſents the channel of a river; A D high water-mark; and G H low water-mark; Z I a paſſage from the river to the cavity I K L M N, or firſt or hidden *Tantalus*; L M Q the ſyphon of the firſt *Tantalus*, running into the ſecond *Tantalus*, or viſible pond O Q R P, which by its ſyphon R S V runs out into low grounds, that may be above the low water-mark G H; and the bottom K L of the firſt *Tantalus* may be above the top of the laſt, whoſe level is the line W W; A B C D Y O Q R P V H is the ſection of the ſurface of the earth.

The Body of a Man *found in a* Copper Mine; *by M.* Adam Leyel. Phil. Tranſ. N° 384. p. 136. *Tranſlated from the* Latin.

IN *December* 1719, in the *Wredian* ſhaft, 82 fathoms deep, was found the body of a man, 5 fathoms under water, and the ruins of a rock; which falling in upon him had cruſhed both his legs, his right arm and head; but his face and the reſt of his body and his cloaths were whole and entire: A Fig. 1. Plate XII. repreſents him with his left hand ſtopping his mouth with the end of his neck-cloth; his purſe and a long braſs-box, in which was a piece of tobacco, were found and entire; but the water impregnated with vitriol had entirely conſumed the piece of iron wire on which the lid of the box turned. His fleſh and ſkin tho' they felt rough and hard to the touch were not petrified, but reſembling horn or hoof; for, it might be cut with a knife.

After the body was taken up, there was diligent ſearch made to find if any knew the man, or the time he periſhed; when one *Magnus Johannis*, a miner in *Korſgârden*, knew him very well by face (all the lineaments of which were entire) and added, that his name was *Matthias Iſraëlis*, or, on account of his

PLATE XII

Fig. I.

Fig. II.

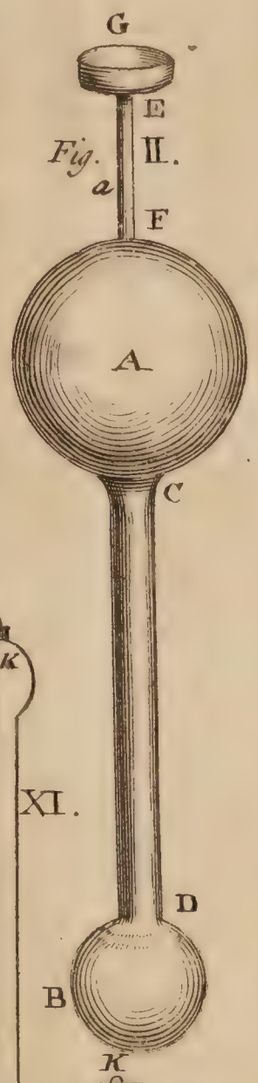


Fig. IV.

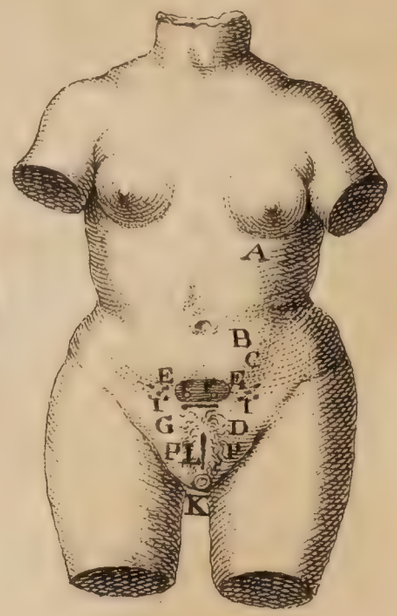


Fig. III.

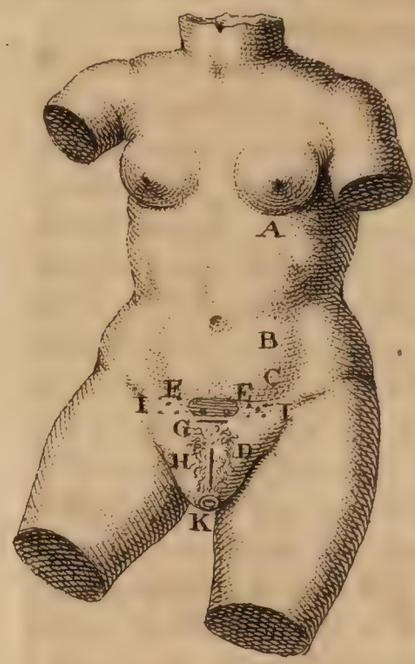


Fig. V.

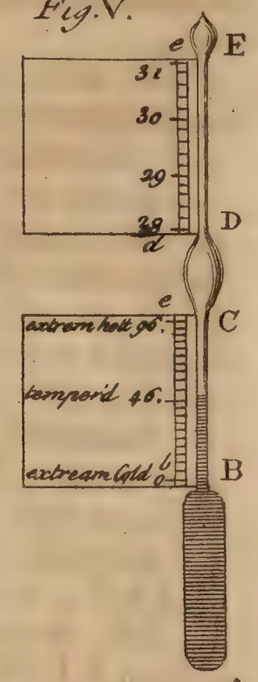


Fig. XI.

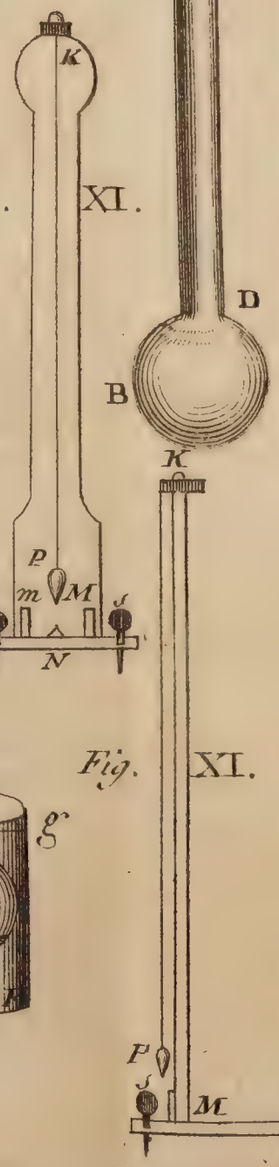


Fig. X.

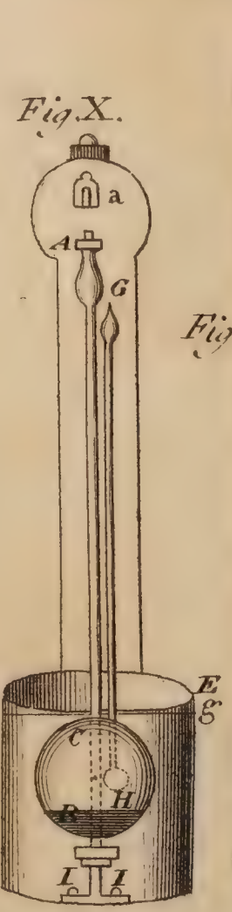


Fig. XII

Fig. VIII.

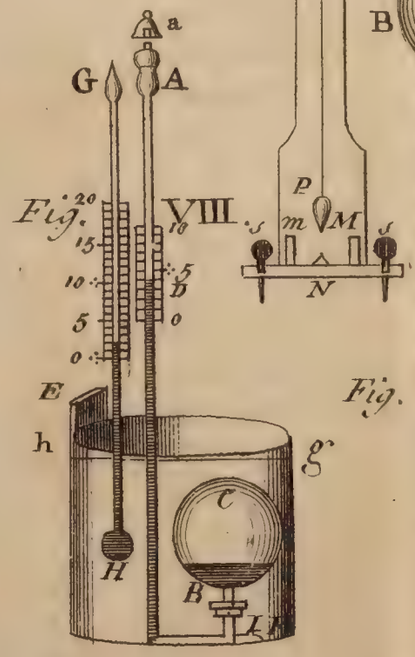


Fig. XIII.

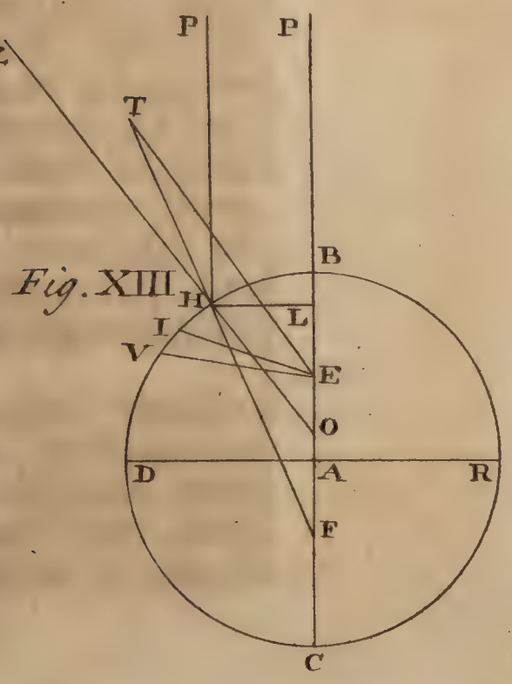


Fig. XIV.

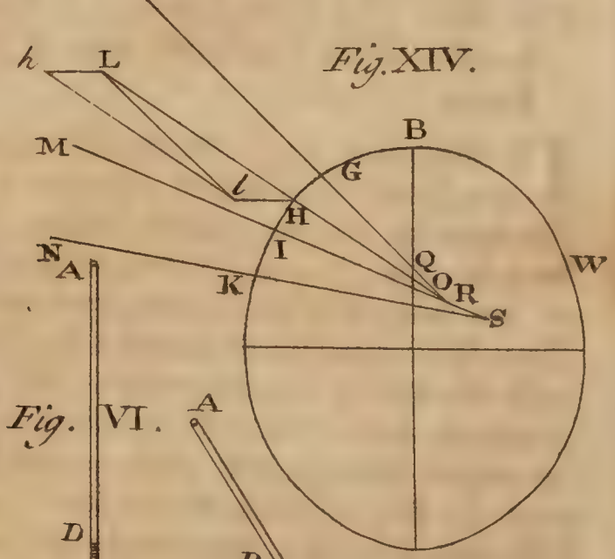


Fig. VI.

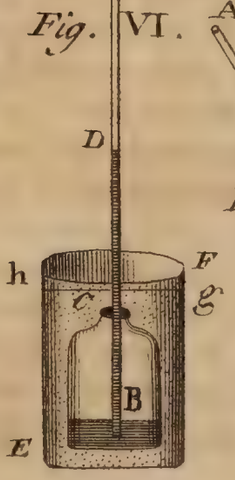
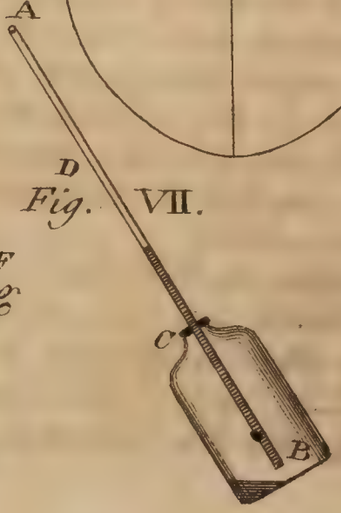


Fig. VII.



his ſtature, *Great* or tall *Matthias*, born at *Boda* a village in the pariſh of *Swerdſiöen*, and employed by *Jonah Petri* in *Dijkarebacken*; and moreover he added, that going down by himſelf in autumn 1670 into the ſhaft, he was afterwards miſſing, and was undoubtedly ſmothered in the ruins; this account was confirmed by ſeveral others, and particularly by an old woman, to whom *Matthias* had been contracted.

He had therefore lain under ground for 49 years, *viz.* from 1670 to 1719; and the body is ſtill preſerved in a houſe belonging to the mines, where to this day it is to be ſeen entire and without any ſtench; not only his cloaths and linen, but his fleſh, ſkin, hair and nails, were preſerved from putrifaction by means of the water impregnated with vitriol.

It appears from this account, as given by the people living thereabouts, that the body was not petrified, but indurated by the vitriolic water: For, nothing is more contrary to the nature of vitriol than a petrifying quality; but by the briſk motion of a very fine vapour it pervades, conſtringes and preſerves every thing from putrifaction.

The Deſcription and Uſe of a New Areometer; *by M.* Fahrenheit. Phil. Tranſ. N° 384. p. 140. *Tranſlated from the* Latin.

THE two tubes C D and E F (Fig. 2. Plate XII.) are joined to the ball A which is pretty large (the larger the better) and a receptacle G to the ſmaller tube E F, and the middle of the tube is diſtinguiſhed by a very ſmall point *a*, yet ſufficiently diſcernible; the other extremity of the tube C D is furniſhed with a ball B, which ſerves inſtead of a receptacle to the inferior weight, with which the inſtrument is charged: Let the diſtance of the ball B from the centre of the ball A be thrice greater than the diſtance of the receptacle G from the ſame centre. The inſtrument being thus prepared, let the ball B be filled with ſo much mercury, that if the areometer be immerged into the lighteſt liquor, as for inſtance, ſpirits of wine, well dephlegmated, or ſpirits of turpentine, it may deſcend therein almoſt to the point *a*; after which, the tube is hermetically ſealed near E, and the inſtrument weighed in a nice pair of ſcales; and the weight of the inſtrument will be the very ſame with that of the liquor, excluded by the inſtrument, as is well known from hydroſtatics. But if the weight of heavier liquors, as water, *lixivia* or acid ſpirits, be ſought, their difference of gravity is found, when the inſtrument is charged with

ſo much weight in the receptacle G, as may make it deſcend again to the point *a*: Upon adding this weight to that of the inſtrument, the ſpecific gravities of theſe liquors (if the weights be very ſmall) will be had ſufficiently exact; and ſo of the reſt.

M. *Fahrenheit* affirmed that the inſtrument ſhould deſcend in the abovementioned ſpirits almoſt to the point *a*; but it will be better, that the liquor do not exactly reach that point, and that the inconſiderable difference be made up by very ſmall weights; for, in this manner, were there ſtill lighter liquors propoſed or were the gravity of the abovementioned liquors rendered ſpecifically lighter by heat, they might ſtill be diſcovered by the inſtrument, which otherwiſe would not happen, ſhould it deſcend in the ſaid liquors exactly to the point *a*.

In making the experiments, care is to be taken, that neither the ſuperficies of the inſtrument or of the liquors have any fat, or other heterogeneous particles adhering to them; elſe the experiments can never be accurately made.

A preternatural Structure of the Parts of Generation in a Woman; *by Mr.* John Bonnet. Phil. Tranſ. N° 384. p. 142.

AS there appears to be ſome difference in the accounts Dr. *Huxham* and Dr. *Oliver* gave of the parts of generation in this woman *Phil. Tranſ.* N° 379. Mr. *Bonnet* ſurgeon at *Foye* in *Cornwall* gives a particular account of what he obſerved, with reſpect to this matter, having ſeveral times obſerved the wonderful ſtructure of the parts both before the birth approached, and before he was obliged to make the inciſion.

This woman was about 23 years of age, when ſhe was married, and ſome time after, ſhe conceived. As ſhe was conſcious of the preternatural ſtructure of the parts and her mother apprehenſive of the danger that would attend the delivery under ſuch unhappy circumſtances, they applied to Mr. *Bonnet* about the ſeventh month in order to engage his aſſiſtance. Upon viewing the *abdomen*, he made the following obſervations: There was no ſign of the *umbilicus*; but about three inches lower than it is regularly placed in perſons naturally formed, there was a ſpongy fleſhy protuberance, nearly of the ſhape and bigneſs of a hen egg; not, as in Dr. *Oliver*'s account, compoſed of ſeveral lobules, envelopped in diſtinct membranes, but entirely reſembling that luxuriant fleſh, in ill-digeſted wounds, and therefore, commonly called proud fleſh. This was exceeding

ing tender, and could not bear the leaſt touch: On the lower part of this excreſcence, he perceived two ſmall orifices about an inch diſtant from each other; thro' theſe the urine continually ouzed, nor could ſhe contain it, but by violent efforts could make it ſpout out near a foot. What Dr. *Oliver* ſays of its being rendred in ſeveral ſmall ſtreams is certainly falſe: The two orifices by which it really was diſcharged, being now very evident, and eaſily admitting a ſmall probe. About $\frac{1}{4}$ of an inch below this protuberance, was a tranſverſe orifice, very much reſembling the *anus* of a cock; thro' this the *menſes* regularly flowed; by this ſhe was impregnated. It was with ſome difficulty Mr. *Bonnet* thruſt his finger into this orifice, in order to reach the *os tineæ*, which, however, he could by no means feel, it lying ſo deep; but he plainly felt a thick tranſverſe membrane, ſeparating this paſſage from an orifice, ſituated about two inches below that already deſcribed. This lower orifice ſeemed to be ſituated exactly at the *ſymphyſis* of the *oſſa pubis*, in women regularly formed, ſomewhat above the place where the natural *hiatus* ſhould have been; he could but juſt enter the tip of his finger into this; there were a few hairs ſcattered irregularly up and down about this orifice. The *anus* terminated as uſual with a *ſphincter*, about two inches below this lower orifice, much more forwards than uſual; ſo that the upper orifice, which may be properly called the orifice of the *vagina*, was about $\frac{1}{4}$ of an inch below the umbilical excreſcence: The lower oblong orifice (or another paſſage to the *uterus*) was about two inches below that of the *vagina*, contrary to what is aſſerted in Dr. *Oliver*'s account, who makes the ſuperior orifice to be near four inches below the excreſcence, and the lower orifice four inches from the ſuperior; and then affirms, that the *anus*, which the Dr. (rightly enough) makes to uſurp the place of the *foſſa magna* in women naturally conformed, was five or ſix inches below this: According to this proportion, the woman muſt have been really gigantic; whereas, ſhe was a ſhort little woman; nor do Dr. *Oliver*'s cuts any ways repreſent the caſe. She had no *os* or *oſſa pubis*; it is true, there was an *apophyſis* jutting out from the lower part of each *os ilium*; but they were far from being joined, as uſual, by *ſynchondroſis*.

July 18, 1722. Mr. *Bonnet* was ſent for late at night; He found the woman with true travail-pains upon her; the throws were exceſſive violent, and the continued agony had almoſt

quite exhauſted her ſpirits; but the orifice of the *vagina* was no ways ſenſibly dilated; tho' the *anus*, thro' the violence of the throws, opened exceeding wide: Vain were all endeavours, by thruſting up the child, and putting the mother in a proper poſture: Vain were her own throws and agonies; convulſions now had ſeiſed her, and nature ſeemed to have denied a longer life to the mother, or an entrance into it to the child. Mr. *Bonnet* was in the utmoſt perplexity what to do under theſe circumſtances; on the one hand he conſidered, that if there was not a paſſage made for the child, and that by inciſion, both mother and child muſt ſpeedily periſh; on the other hand he foreſaw the hazard of an inciſion: At laſt he reſolved to try a doubtful method of preſerving life, rather than none at all. He told her mother and the other perſons in the room, that death was inevitable, without widening the paſſage by inciſion, and ſo attempting a delivery; withal apprizing them of the uncertainty of the event of the operation. When they ſaw her, as they thought, juſt expiring, they delivered her into his hands, to do with her what he thought fit. He immediately thruſt his ſcalpel into the inferior oblong orifice of the *vagina*, and directing it towards the ſuperior, he brought both into one; then preſently with his ſciſſars, he ſnipped the tranſverſe membrane: This being done, he eaſily introduced his hand, felt the head of the child, and thruſting his finger into its mouth, he extracted a female infant, alive and well formed, to his own ſurpriſe and that of all the aſſiſtants.

Ever after, ſhe was troubled with a *prolapſus uteri*, upon the leaſt ſtanding or walking: Mr. *Bonnet* propoſed to remedy this by a future, as is practiſed in the caſe of the *vulva* breaking into the *anus*; but ſhe would by no means admit of it. So that ſhe almoſt continually laboured under a *procidentia uteri*, and the body of the *uterus* and *vagina* were ſo corroded by the acrimony of the urine, that there were four or five ulcers form'd upon them. Beſides this inconvenience, ſome of the thinner parts of the *fæces* were diſcharged at the bottom of this large *rima*, and by introducing his finger at the bottom thereof, he could eaſily thruſt the top of it thro' the *anus*; which ſhews, that there could not be five or ſix inches between the oblong orifice and the *anus*, as is aſſerted by Dr. *Oliver*; the diſtance being ſcarce above two inches.

A (Fig. 3. Plate XII.) repreſents the *epigaſtrium*; B the umbilical region; C the *hypogaſtrium*; D the *regio pubis*;

EE the ſpongy maſs; G the tranſverſe orifice; H the oblong orifice; II two cicatrices, occaſioned, probably, by the acrimony of the urine; K the *anus* placed a little forwards.

FF (Fig. 4.) repreſents the urinary paſſages, which were diſcernible enough after the delivery; P P the *fiſſura magna* four inches long; L the *procidentia uteri*; M the *os tineæ*.

A Partial ſight of Objects; *by Dr.* Abraham Vater. Phil. Tranſ. N° 384. p. 147. *Tranſlated from the* Latin.

A Middle aged woman, was in one night ſeiſed with a black cataract or *gutta ſerena*, by the retropulſion of a *coryza*, by bathing and a ſubſequent cold: For, getting up in the morning, ſhe found herſelf deprived of her ſight, without any external blemiſh in her eyes. After taking ſeveral laxative medicines and purifiers of the blood, and at the ſame time applying veſicatories, and uſing a regular diet, her ſight gradually returned; yet with the following particular phænomena: At firſt all the people ſhe met, appeared without heads, their bodies only without the head preſenting to view. In proceſs of time ſhe ſaw objects entire, but thro' a miſt or net, as it were; and after this, ſhe obſerved ſpots or mots before her eyes: At length this diſorder degenerated into a partial ſight: And now ſhe ſaw an object entire, with both her eyes open and directed towards it; but ſhutting either of them, part of the object ſeemed to be covered with a round ſpot or miſt, which appeared ſo much the larger, as the eye was farther removed from the object: But as either the right or left eye was ſhut, the middle (tho' different) part of the objects was obſcured: Thus, for inſtance, if ſhe view'd with her left eye only theſe three words *ego ſum cœca*, the middle word *ſum*, when the pupil was directed towards it, was not ſeen, but only the words *ego — cœca*; and when the pupil was directed towards *ego*, it was not ſeen, only the words *— ſum cœca*: But if on the contrary ſhe ſhut her left eye and viewed an object with her right, then it is true, the middle of the object was not ſeen; yet in ſuch a manner, that only a fourth part of the object eſcaped the ſight, while the other three parts were diſtinctly ſeen. Thus, for inſtance, ſhould ſhe view with this eye theſe four words *ego opto eſſe ſana*, then the pupil being directed towards the middle, ſhe ſaw all the words but *opto*, which was obſcured by a round ſpot, and with a great ado ſhe ſaw *ego — eſſe ſana*.

Since, therefore, it appears from this caſe, that the round ſpot, by which part of the object was obſcured, was really fixed,

fixed, tho' changing its place according to the different direc tion of the pupil, it ſeems doubtful, whether the defect was i the cryſtalline humour or in the *retina?* In the *cornea* there ap peared no ſpot or obſcurity.

The abovementioned phænomenon, *viz.* where ſhe could ſe the whole body of a man, excepting only his head, ſeems to b of greater conſequence and more difficult to be explained.

The Diſſection of two Eyes *with* Cataracts; *by Mr.* Moly neux. Phil. Tranſ. N° 384. p. 149. *Tranſlated from the* Latin.

ONE *John Wright*, an old ſoldier, having a cataract in both his eyes, at length loſt his ſight entirely. He appli ed to Mr. *Caywood*, occuliſt in *Dublin*; who couched both his eyes, in order to depreſs the cryſtalline humour: But the opera tion not ſucceeding ſo well on his right eye, he only recovered the uſe of his left. A few months after, he was taken into the King's hoſpital for decay'd ſoldiers near *Dublin*, where he con tinued for eight or nine years, and during all that time, his ſight was pretty good, ſo that he could walk by himſelf thro' the city, and diſtinguiſh the faces of his comrades. At length he died *April* 5, 1722.

Upon removing the *cornea* with a part of the *ſclerotica* of the left eye (the ſight of which he had recovered, as above) there was not the leaſt ſign of a pellicle, either floating in the aqueous humour, or adhering to the edge of the *iris*, as *Wol houſius* affirms he had obſerved; and what is ſtill more ſurpri ſing, there was nothing remaining of the cryſtalline humour, to whoſe opacity Dr. *Briſſeus* aſcribes this diſorder: Nothing preternatural was obſerved in the ſite and conſiſtence of the vi treous humour; the *choroides* and *retina* were of a duſkiſh co lour both internally and externally: The ſame things were ob ſerved in the right eye, and no ſigns of a pellicle or cryſtalline humour: The *cornea* was rugous and flaccid; becauſe the aqueous humour, extravaſated by the puncture, had never been renewed, and therefore, the patient loſt the uſe of that eye entirely: The want of the cryſtalline humour, is a thing quite unheard of, no anatomiſt, as far as Mr. *Molyneux* knows, having ever obſerved or even ſuſpected any ſuch thing. He doubts not, but the patient had the cryſtalline humour in both eyes before the operation; but he ſuppoſes, by its being de preſſed by the needle, and thereby the *ligamenta ciliaria* and veſſels, by which it was nouriſhed, being entirely broken, it gradually

gradually wasted away. It were to be wished, that this matter were confirmed by repeated experiments: For, Mr. *Molyneux* suspects, that the same thing always happens in a certain time after depressing the crystalline humour.

From this account it is evident, that a person may see without the crystalline humour, contrary to the opinion of *Wolhousius*, and by it is farther confirmed the opinion of Dr. *Brisseus* concerning a cataract.

An Account of the Scarabæus galeatus pulsator, *or* Death-watch; *by Mr.* Stackhouse. Phil. Trans. N° 385. p. 159.

MAY 16, 1724. Mr. *Stackhouse* happened to hear what is called the *death-watch*; and as he cast a diligent eye over the sedge bottom of a chair, he hit luckily on the place where the insect was beating; so that it discovered itself to him by its own pulsations.

It lifted up itself upon its hinder legs, and somewhat extending or inclining its neck, it beat down its face upon the sedge, with great force and agility: The sedge, upon which he found it, was bared of its outer coat, for about the length of $\frac{1}{2}$ an inch; the insect stood upon the inner bulbous part, and beat upon the outer coat, as if it had been working it off, as it went; the impressions of its strokes were very visible, the coat of the sedge being depressed, where it had beaten, for about the compass of a silver penny; whether it beat for exercise or food Mr. *Stackhouse* could not tell; but very probably it might be for the latter; and he is rather inclined to think so, because there were more than one such place upon the sedge, where it had been at work; and where, it is likely, it might have continued for some days.

As to what Mr. *Derham* has observ'd, *viz.* that the beatings are a sort of prelude for copulation, Mr. *Stackhouse* could not discover that this beetle had any other of the same species near it; and, therefore, he is inclin'd to think, that it beat for the preparation of its food, at this time at least, whatever it might do at other times for pleasure. The description Mr. *Allen* gives of the insect, as far as Mr. *Stackhouse* could find by this (which he took from the chair where he found it, and put it into a box) is very true: It is about $\frac{1}{4}$ of an inch in length, of a dark dirty colour, with a broad helmet over its head, under which, when quiet, it draws up its head; so that this helmet or *galea* is, when the insect rests, a very considerable defence against such falls as are frequent in rotten

and

and decay'd places, which this inſect ſeems to frequent very much: The ſecond day after he took it, he opened the box wherein it was, and ſet it in the ſun: The inſect ſoon became very briſk, and crept backwards and forwards along the pieces of ſedge and rotten wood, he put with it into the box; till at length getting to the end of one of the pieces, it immediately ſtruck out its wings, and was juſt going to take flight; but having the lid of the box ready in his hand, he ſhaded it over and it ſoon drew in its wings and was very quiet. He could not before perceive, tho' he made uſe of a tolerable good glaſs, any the leaſt ſign of a fiſſure upon its back; and for that reaſon he queſtioned much, whether it had any wings or not, till he ſet it in the ſun. The head of this inſect appears to be of a very fine contexture, as it is ſeen, when it creeps about, and ſtretches it forward; but when it is drawn up under its helmet, it ſeems to be cover'd with a membrane thick ſet with fine hairs.

It liv'd about a fortnight; but he could never perceive that it beat, after it was confin'd in the box.

A new Contrivance for taking Levels; *by Dr.* Deſaguliers. Phil. Tranſ. N° 384. p. 165.

IT has been long ago obſerv'd, that the air thermometer is alſo a barometer; and becauſe the liquor in it will riſe and fall, as well by the change of the weight of the air, as by the rarefaction of the air by heat and cold, this inſtrument has been no longer made uſe of as a thermometer; and in its ſtead, ſpirit-of-wine thermometers, hermetically ſeal'd, have been us'd ever ſince.

But becauſe the errors of the air thermometers (or its difference from the ſpirit thermometer) depend only upon the change of the weight of the atmoſphere from what it was, when the two thermometers were ſet at the ſame degree of their reſpective ſcales; the late Dr. *Hook* contriv'd an inſtrument, which he call'd a marine barometer, made of a combination of the two above-mentioned thermometers; in ſuch manner, that a third ſcale being made uſe of, to obſerve the difference of the two thermometers, thereby the change of the gravity of the air; and conſequently, ſtormy, rainy, and fair weather might be foretold at ſea, where the quickſilver barometer becomes uſeleſs by the ſhaking of the ſhip.

Dr. *Halley* has in the *Philoſophical Tranſactions* propos'd Mr. *Patrick*'s pendent barometer for taking the level of diſtant

tant places; becauſe the mercury, in the tube of the ſaid barometer, does ſometimes riſe and fall a foot, or a foot and a half; if therefore, the motion of the mercury in this barometer be five times more ſenſible than in the common one, $\frac{1}{10}$ of an inch of fall of the mercury will anſwer to an height of 18 foot; and therefore, ſuch an inſtrument might be of uſe in taking the levels of diſtant places. But Dr. *Deſaguliers* found by ſeveral experiments, that this will not anſwer in practice; becauſe as the tube of ſuch a barometer is of a very ſmall bore, the attraction of coheſion, by which the mercury is apt to adhere to the tube, will diſturb the motion of the mercury, caus'd by the different preſſure of the atmoſphere; ſo that ſetting up this barometer ſeveral times ſucceſſively in the ſame place, it will often differ $\frac{1}{10}$ of an inch or more; and if it be ſhaken, as is commonly done to ſet it right, the mercury will ſometimes part, and a drop of it fall from the reſt; ſo that it is leſs to be depended upon for this uſe, than the common barometer.

Mr. *Stephen Gray* has often made a very ſenſible barometer. Into a little bottle C B (repreſented Fig. 5. Plate XII.) he fixes a tube A B of a very ſmall bore, open at both ends, and cemented tight to the neck of the bottle at C; then warming the bottle with the hand, in order to drive out ſome of the air, he immerges the end A into water, tinged with cochineal; ſo that as the air cools in the bottle C B, ſome of the red water is forced into the bottle; then ſetting the bottle upright again, the liquor in the bottle will ſtand at B, (above the end of the tube) and that in the tube at D; but if it ſhould ſtand higher or lower than D, it may be brought to that place by ſucking or blowing at A. If the inſtrument thus prepar'd, be firſt ſet on the ground, and a ſpringing ring of fine wire ſlipp'd on the tube down to D, by way of index, and then ſet upon any table, or other place, ſcarce a yard higher, one may obſerve that the liquor is riſen ſenſibly. The Dr. has obſerv'd it riſe $\frac{1}{4}$ of an inch, when the bottle was ſet but a yard higher than where it ſtood before; ſo that the column of atmoſphere, that preſs'd down the tube, whilſt the machine was on the ground, being ſhorten'd only three foot, was ſo overbalanced by the expanſion of the air in the bottle at B, that the liquor roſe $\frac{1}{10}$ of an inch above D. There is, it is true, a great uncertainty in this inſtrument: For, ſince it is a thermometer, as well as a barometer, the warmth of the hand that touches, or even comes near it, will make it riſe,

if the air in the bottle were cold before. Mr. *Gray*, therefore, contrived to put the bottle C B into the veſſel F E, which he fill'd with ſand, that in raiſing the inſtrument, and moving it up and down, the air in C B might continue in the ſame ſtate, and the machine be only a barometer during the experiment.

This ſeems to bid fair for an inſtrument, by which the different levels of places may be taken; but upon a nice examination it will be liable to error: For, tho' ſand be not alter'd in its heat or cold of a ſudden; yet in two or three hours, as it is carried into a warmer or colder place, it will become hotter or colder, and the leaſt degree of heat or cold, communicated to the air C B, will alter the height of the liquor at D, when the inſtrument is made ſo ſenſible, as the Dr. has mentioned. Then, if in carrying the inſtrument, it ſhould be accidentally inclin'd; ſo that the liquor in the bottle (repreſented Fig. 6.) ſhould not cover the bottom of the tube at B, ſome liquor may fall out of the tube, or ſome air may inſinuate into it; each of which accidents will quite ſpoil the experiment. But if this machine be made portable, without any inconveniency, and ſecur'd againſt the action of heat and cold (or which is the ſame thing, if the alterations by heat and cold be exactly allow'd for) it will be of very great uſe and certainty, in taking the levels of diſtant places, provided they be not ſo far diſtant from each other, as to require upwards of 6 hours time to carry the inſtrument from one place to another: Nay, very diſtant places, even at two or three days journey from each other, may be taken tolerably well with two inſtruments, nicely adjuſted to each other, if they be taken notice of by two obſervers at the ſame hour, in fair weather, and when there is no wind.

Now, Dr. *Deſaguliers* contriv'd ſuch an inſtrument, by which the difference of level of two places, which could not be taken in leſs than four or five days with the beſt teleſcope levels, may be taken in as few hours.

To the ball C (Fig. 7.) is join'd a recurve tube B A of a very fine bore, with a ſmall bubble a-top at A, whoſe upper part is open. It is evident from the make of this inſtrument, that if it be inclin'd in carrying, no prejudice will be done to the liquor, which will always be right, both in the ball and tube; when the inſtrument is ſet upright: If the air at C be ſo expanded by heat, as to drive the liquor to the top of the tube, the cavity A will receive the liquor, which will come down

down again and ſettle at D or near it, according to the level of the place, where the inſtrument is, as ſoon as the air at C returns to the ſame temperament, as to heat and cold. To preſerve the ſame degree of heat, when the different obſervations are made, the machine is fixed in a tin veſſel F E, fill'd with water up to *g h*, above the ball; and a very ſenſible thermometer has alſo its ball under water, that one may obſerve the liquor at D in each experiment, when the thermometer ſtands at the ſame height as before. The water is pour'd out, when the inſtrument is carried, which one may do conveniently, by means of the wooden frame, which is ſet upright, by means of three ſcrews, ſuch as *s* (Fig. 8.) and a line and plummet *p* P. At the back part of the wooden frame, from the piece at top K (Fig. 10.) hangs the plummet P, over a braſs point at N; M *m* are brackets to make the upright board K *n* continue at right angles, with the horizontal one at N. Fig. 11. likewiſe repreſents the wooden frame and ſcrews. Fig. 9. repreſents a front view of the machine, ſuppoſing the forepart of the tin veſſel tranſparent; and here the braſs ſocket of the recurve tube, into which the ball is ſcrew'd, has two wings at I I, fix'd to the bottom, that the ball may not break the tube by its endeavour to emerge, when the water is pour'd in as high as *g h*.

After the Dr. had contriv'd this machine, he conſider'd, that as the tube is of a very ſmall bore, if the liquor ſhould riſe into the ball A, in carrying the inſtrument from one place to another, ſome of it would adhere to the ſides of the ball A; and that upon its deſcent in making the experiment ſo much might be left behind, that the liquor would not be high enough at D, to ſhew the difference of level; therefore, to prevent that inconveniency, he contriv'd a blank ſcrew to ſhut up the hole at A as ſoon as one experiment is made, that in carrying the machine, the air in A may balance that in C; ſo that the liquor ſhall not run up and down the tube, whatever degree of heat and cold may act upon the inſtrument, in going from one place to another.

Now, becauſe one experiment may be made in the morning, the water may be ſo cold, that when a ſecond experiment is made at noon, the water cannot be brought to the ſame degree of cold, it had in the morning; therefore, in making the firſt experiment, warm water muſt be mix'd with the cold; and when the water has ſtood ſome time, before it comes to be as cold, as it is likely to be at the warmeſt part

of that day; obſerve and ſet down the degree of the thermometer, at which the ſpirit ſtands; and likewiſe the degree of the water in the barometer at D; then ſcrew on the cap at A, pour out the water, and carry the inſtrument to the place whoſe level you would know; there pour in your water; and when the thermometer is come to the ſame degree as before, open the ſcrew at top, and obſerve the liquor in the barometer.

The Dr's ſcale for the barometer is 10 inches long, and divided into tenths; ſo that ſuch an inſtrument will ſerve for any heights, not exceeding 10 foot, each tenth of an inch anſwering to a foot of height.

N. B. The Dr. has made no allowance for the decreaſe of denſity in the air; becauſe he did not propoſe this machine for meaſuring mountains (tho' with a proper allowance for the decreaſing denſity of the air it will do very well) but for heights that want to be known in gardens, plantations and the conveyance of water; where an experiment, that anſwers to two or three foot in a diſtance of 20 miles. will render this a very uſeful inſtrument.

The bones of a Fœtus *voided by Stool; by M.* Bernard Shiever. Phil. Tranſ. N° 385. p. 172. *Tranſlated from the* Latin.

A Woman of 41 years of age, conceived in *July* 1720, and having gone ſeven months with child, tho' ſometimes ſhe had her *menſes* in a ſmall quantity, ſhe perceiv'd her belly leſſen, with only a kind of preſſure remaining in her right ſide. A month after, ſhe conceiv'd again, and in *December* 1721 was deliver'd of a dead female child, of a proper ſize. From that time ſhe kept her bed till *June* 1724. In *May*, happening to go to ſtool, ſhe felt a pain in the *anus*, as if the *rectum* would drop from her; endeavouring with her fingers to relieve herſelf, ſhe extracted a piece of the *crainum*, as big as a *Swediſh* crown, call'd a *dubble carolin*; and at the ſame time two ribs were found in the cloſe-ſtool; and 14 days after, the reſt of the bones, (of an excrementitious colour) were voided the ſame way, which M. *Shiever* himſelf ſaw and handled. The woman did afterwards very well, went about her family affairs, and was the mother of three children, ſhe alſo had her *menſes* naturally.

An

An Account of an Aurora Borealis, *obſerv'd at* Upſal, September 20. 1720; *by M.* Burrman. Phil. Tranſ. N° 385. p. 175. *Tranſlated from the* Latin.

THE *chaſmata cæli*, as they are call'd, or the *lumen horizontale* and *aurora borealis*, in *Swediſh Nord-skjen*, *Nord-ljus*, *Nord-blyſs*, *Nord-blâſs*, *Lâterskjen*, *Lyſſnor*, &c. by the vulgar ſuppos'd to be repreſentations of armies and battles, and frequent in *Swedland* and other places nearer the pole, M. *Burrman* often and diligently obſerv'd. He had an opportunity, when travelling in the night time, of more accurately obſerving an uncommon and very ſingular *aurora borealis*, on *September* 20. 1717, which gave him occaſion to ſubmit a conjecture about the optical nature of the phænomenon, to the conſideration of the learned.

The tracks of theſe *chaſmata* were more numerous than uſual, *viz.* four or at leaſt three whitiſh arches, brighter than the reſt of the ſky (which was clear and calm) divided by ſmall intervals of darkneſs, and placed one over the other. What was a very uncommon, and at the ſame time an agreeable ſight was, that the diſtances of the ſaid arches, greateſt under the *Bears* (below which the higheſt was not above 26 degrees, *Dubbè* a ſtar of *Urſa Major* at times ſhining thro' it) gradually decreas'd on both hands towards the horizon, till they at length interſected each other in the very points of eaſt and weſt, juſt as the meridians on an artificial globe converge towards the poles of the equator. Theſe white arches or ſemicircles for the moſt part conſiſted of diſtinct *ſtriæ*, perpendicular to the horizon, eſpecially in the vertex, or under the north; a fainter and more obſcure light was obſerv'd on the ſides.

The *ſtriæ* had a double motion, the one horizontal, the other perpendicular; the latter was much leſs and ſlower; ſo as ſcarce to diſturb the circular form of the arches; the former was various, and at the ſame time very ſwift, from eaſt to weſt, and back again. And as often as ſeveral *ſtriæ* (which frequently happened) coming from different parts, met each other, whether in the middle of an arch or elſewhere, a very agreeable variety of colours was obſerved to proceed from this commixture or various interſection of the rays, and that in the ſame order, as the colours are produced in the priſm, the feathers of ſmaller birds, or ſuch like bodies, expos'd to the ſun.

Hence

Hence we may take occaſion of farther conſidering the optical reaſons of the phænomenon, with *Des Cartes cap.* 7. *parag.* 18. *de Meteoris:* But yet M. *Burrman* does not think, that an accenſion of a more ſubtile ſulphureous matter in the inferior region of the atmoſphere is altogether to be denied a place in this matter. For, he himſelf obſerved a much greater variety of colours, with a hiſſing noiſe, like the flame of a fire, at ſeveral other times, but eſpecially in the *Chaſma* of *Mar.* 17, 1716. which was more remarkable in *Swedeland*, for a whole night together, than in *England*, *France*, *Germany* or any where elſe. We muſt, perhaps, ſuppoſe two different kinds of *aurora borealis*; the one a fiery meteor, from exhalations and effluvia; the other merely optical from the different refraction and reflexion of the ſolar rays, either in ſome icy *lamellæ*, or icicles, poſſeſſing the higher regions of the atmoſphere, or ariſing from ſome of the northern ſeas, and thence communicated to us from the clouds. Indeed, the latter ſeems to be illuſtrated by a particular experiment, invented and tried on occaſion of the already deſcribed phænomenon (yet ſomething reſembling this M. *Burrman* likewiſe obſerved in *February* 1716, at nine o'clock in the evening, conſiſting of two ſuch ſemicircular arches, but with fewer *ſtriæ* and higher above the northern part of the horizon) and is, as follows.

If a plate of tin be taken of any length and breadth, and at one ſtroke ſcraped lengthwiſe with a ſharp and ſtrong knife, till it become full of *ſtriæ*; and held in the hand in ſuch a poſition, that its plane may form equal angles with a lighted candle, and a dark wall or table; and then be bent and incurvated different ways, at one time turning its concave ſide and at another its convex ſide to the wall or table, faſter or ſlower at pleaſure; the appearances will be very like the abovementioned, and afford no leſs agreeable a ſight.

But it may happen that ſometimes, both theſe kinds of light exiſt together, and coincide with each other, ſo as that neither is the cauſe or effect of the other; but both concur to enlighten the night and ſtrike terror into the beholders. For, as M. *Burrman* has very frequently obſerved this horizontal light, *viz.* flaming zones, at one time ſingle (nay, paſſing almoſt over the zenith, which the common people, on account of the reſemblance, uſually confound with the *via lactea*, as being but a little broader) and at another time attended with columns, inverted pyramids and a variety of other figures, but without any accenſion of the air; ſo this latter frequently happens without theſe appear-

ances

ances distinctly observed, either preceeding or accompanying it: Tho' it be likewise true, that from some quarter of the heavens, at first only flaming, there at length dart up to the zenith and sometimes over all the hemisphere very strong lights, either by the reflection of the rays of the sun in the icy particles of the ocean, or in some caustic mirrors, or accended some other way.

But since no one can with certainty discover the true causes of this surprising phænomenon, previous to a due comparison of the several observations taken at the same time in different places of the earth; it may more especially appear, whether that light be seen under the same angle of altitude even in distant places; and whether what in one place is parallel to the horizon, be not in another perpendicular thereto; in short, whether it be one and the same arch that is observed in different places; or whether here, as in the rainbow, there are not as many different arches as different spectators: M. *Burrman* therefore earnestly intreats all, who have any regard for the advancement of natural knowledge, but more especially those in the more northerly parts of the world, carefully to observe this phænomenon, as to all its circumstances, and communicate their observations either to the public or to him, which he thinks would be a very singular favour done the learned world.

A New Barometer; *by M.* Fahrenheit. Phil. Trans. N° 385. p. 179. *Translated from the* Latin.

TO the cylinder A B (represented Fig. 12. Plate XII.) is joined the tube B C, to which an oblong ball C D is added, and to this the tube D E of a very fine bore. The cylinder, which can bear the heat of boiling water, is filled with a certain liquor; the sensible degrees of heat in the air are measured in the tube B C by means of the scale *b c*. If this thermometer be put into boiling water, the liquor therein will not only fill the ball C D, but likewise rise to the different limits of the tube D E, according to the degree of heat, which the water, at the time of making the experiment, will acquire from the gravity of the atmosphere. So that, for instance, if, at the time of making the experiment, the height of the mercury in the barometer be 28 *London* inches, the liquor in this thermometer will reach the lowest place in the tube D E: But if the gravity of the atmosphere be equal to the height of 31 inches of mercury, the liquor will be raised by the heat of the boiling water to the highest part of the tube D E; by means of the annexed scale *d e*, the different limits of the heat of the boiling

water

water will be denoted, not by degrees, but by the number of inches, by which the height of the mercury in barometers is commonly measured.

An Eclipse of the Moon, Nov. 1, 1724. N. S. *by F.* Carbonee *and* Dominico Capasso. Phil. Trans. N° 385. p. 180. *Translated from the* Latin.

F. *Carbone* and *Dominico Capasso* observed this eclipse with two telescopes, the one eight *Paris* feet in length, and very clear, the other ten, but not so clear; yet the lunar *maculæ* were very distinctly discerned thro' both: To measure the time, they made use of a pendulum clock, pretty accurate, fixed several days beforehand at the place of observation, and by repeated trials reduced as near as possible to the mean motion of the sun, by a meridian line, they had drawn there some time before, and several times examined. The very night of the eclipse, the clock was thrice examined, in order to find its difference from the true time; first, by a transit of *Fomahant* over the meridian, at $8^h\ 17'\ 18''$; and then by a transit of *Rigel* on the foot of *Orion* at $2^h\ 35'\ 21''$; and lastly, by a transit of *Sirius* at $4^h\ 7'\ 40''$. The *R. Ascen.* were taken from *De la Hire*'s tables. They found the clock go only seven minutes too slow, which are added to the time of the following observation.

From sun-set to midnight the sky was overcast with clouds and rain; but a wind about one o'clock rendered it pretty clear, and it continued so till near three o'clock.

True time h.	′	″	
1	38	00	The *penumbra* begins to be sensible.
1	41	00	It becomes thicker.
1	43	29	It becomes very thick.
1	47	45	The shadow begins.
1	49	25	The disk of the moon appears eclipsed.
2	0	16	The shadow at *Aristarchus*.
2	0	39	At *Plato*.
2	1	10	*Aristarchus* entirely immerged.
2	6	22	*Archytas*.
2	8	7	*Aristoteles*.
2	10	29	*Pitheas*.
2	11	28	*Galilæus*.

True

True time h.	′	″	
2	13	22	The ſhadow at the eaſtern ſhore of *Mare ſerenitatis.*
2	15	34	*Endymion* entirely immerged.
2	18	2	*Copernicus* begins to immerge.
2	20	7	Entirely immerged.
2	21	5	*Poſſidonius* begins to immerge.
2	22	8	Entirely immerged.
2	27	49	*Ricciolus* begins to immerge.
2	31	56	The ſhadow at *Grimaldus.*
2	34	37	At the north ſhore of *Mare Criſium.*
2	37	17	*Proclus* immerges.
2	40	0	A cloud comes on, that quite covers the moon and would continue for ſome time.
3	25	0	The cloud goes off; and now *Grimaldus,* *Ricciolus,* *Keplerus* and *Galilæus* emerged out of the ſhadow.
3	29	2	*Ariſtarchus* emerges.
3	30	30	*Copernicus* begins to emerge.
3	31	34	Entirely emerged.
3	39	18	*Pitheas* emerges.
3	47	46	*Timocharis.*
3	54	57	*Archimedes.*
3	57	18	*Plato* begins to emerge.
3	58	59	Entirely emerged.
4	2	0	The moon again overcaſt with a ſmall cloud.
4	6	0	Now ſhe appears again.
4	8	15	*Ariſtoteles* entirely emerged. The moon at times overcaſt with ſmall clouds, the emerſions of the other *maculæ* could not be exactly obſerved.
4	20	36	The end of the eclipſe, obſerved, probably, later by reaſon of a thin vapour that interpos'd.
4	26	0	The end of the thicker *penumbra.*
4	28	0	The end of the ſenſible *penumbra.*

S. *Carbone* and *Capaſſo* endeavoured with all poſſible accuracy to obſerve this eclipſe; being furniſhed by the king of *Portugal* with a great variety of inſtruments.

Eclipſes of Jupiter's *firſt* Satellite, *obſerved at* Lisbon; *by F.* Carbone. Phil. Tranſ. N° 385. p. 185. *Tranſlated from the* Latin.

1723	h.	′	″
Emerſions			
July 23.	7	47	0
September 7.	8	21	48
1724			
Immerſions			
June 8. *Mane*	2	3	28
15.	3	56	27
30.	2	8	51
Emerſions			
September 2.	9	36	57
9.	11	34	26
25.	9	59	21
October 4.	6	26	44
18.	10	21	20
November 3.	8	42	30

The preceeding obſervations were made with S. *Campani's* teleſcope, 30 *Roman* palms in length.

An Eclipſe of the Moon *at* Paris, Nov. 1. 1724. N. S. *by M.* Maraldi. Phil. Tranſ. N° 385. p. 186. *Tranſlated from the* French.

True time. h.	′	″	
2	33	30	The beginning of the eclipſe.
2	46	15	The ſhadow at *Ariſtarchus.*
2	56	20	At *Galilæus.*
3	20	30	At the north ſhore of *Mare Caſpium.*
3	23	30	At *Proclus.*
4	14	30	*Ariſtarchus* emerges.
4	17	50	*Copernicus* entirely emerged.
4	33	34	*Timocharis* emerges.
4	44	23	*Plato* entirely emerged.
5	6	30	The end of the eclipſe.
2	33	0	The duration of the eclipſe.

The

The Longitude *of the* Fort *of* New-York, *determined from Eclipses of the first Satellite of* Jupiter; *together with the* Variation *of the* Magnetic Needle; *by Mr.* William Burnet. Phil. Tranſ. N° 385. p. 162.

MR. *Burnet* formerly determined the Lat. of the fort of *New York* to be 40° 40′.

Auguſt 9. 1723. At *London* the time of the emerſion of *Jupiter*'s firſt ſatellite, according to Mr. *Pound*'s tables, reduced to apparent time $16^h\ 9'\ 25''$
Time, as obſerv'd at *New-York* 11 10 43

Difference of meridians 4 58 42

Aug. 25. Alt. of the ſun's ſuperior limb.	° ′ ″	Time by the clock. h. ′ ″	Time by calculation. h. ′ ″
Sun's declina. 6° 55′	49 30 00	10 17 52	10 17 28
	51 13 30	10 33 10	10 32 8
Auguſt 26.			
Sun's declina. 6° 33′	46 24 0	9 57 40	9 56 25
	47 50 0	10 8 22	10 6 57
Time of emerſion by Mr. *Pound*'s tables			14 31 25
Equation of time to be added.			00 1 22
			14 32 47
Time obſerv'd by the clock			9 35 14
The ſame corrected			9 34 14
The difference of meridians			4 58 33

This Mr. *Burnet* takes to be the diſtincteſt and beſt obſervation.

September 10. Alt. of the ſun's ſuperior limb.	° ′	Time by the clock. h. ′ ″	Time by calculation. h. ′ ″
Sun's declina. 49′	33 21	9 1 0	9 0 16
	34 6	9 6 1	9 4 49
September 17.			
Sun's declina. 1° 54′	17 17	4 21 40	4 21 44
	15 15	4 33 5	4 32 47

September. 10.	h.	′	″
Time of emerſion by the clock	8	10	0
Time of emerſion by Mr. *Pound*'s tables	12	50	36
Equation of time to be added	0	6	54
	12	57	30
Corrected time at *New-York*	7	59	8
Difference of meridians	4	58	22

June 26. Altitude of the ſun's ſuperior limb.			Time by the clock.			Time by calculation.		
June 20.	o	′	h.	′	″	h.	′	″
Sun's declina. {	56	44	9	48	3	9	43	37
23° 7′ {	60	27	10	9	40	10	5	5
June 27.								
Sun's declina. {	63	31	10	27	43	10	27	5
22° 26′ {	65	21	10	40	0	10	39	27

	h.	′	″
June 26. Time of immerſion by the clock	11	41	12
Time of immerſion by Mr. *Pound*'s tables	16	43	2
Equation of time to be ſubſtracted	0	4	26
	16	38	36
Time at *New York* corrected	11	40	15
Difference of meridians	4	58	21

The mean of all theſe obſervations is 4^h 58′ 30″, which to 3″ agrees with that obſervation, Mr. *Burnet* took to be the moſt exact; and therefore, the Long. of *New York* is nearly 74° 57′ 30″ weſt from *London*.

The variation of the magnetic needle was obſerv'd in 1724, to be 7° 20′ weſt. Mr. *Wells* ſurveyor general of *New-York* obſerv'd it in 1686 to be 8° 45′; by which it appears to decreaſe about 1° 25′ in 38 years, or a little more than 2 minutes in a year.

The Longitude *of* Lisbon, Paris *and* London, *determined from the* Eclipſes *of the* Moon, Nov. 1. 1724. *and* Jupiter's *Satellites*; *by F.* Carbone. Phil. Tranſ. N° 385. p. 186. *Tranſlated from the* Latin *and* French.

IT is to be obſerv'd, that the difference between the meridians of *Lisbon* and *Paris*, was now found leſs than what F. *Carbone* imagined; not by new obſervations at *Lisbon*, but by comparing theſe already made there with thoſe at *Paris*; having receiv'd none before, by which he could determine with certainty the ſaid difference; but he only compar'd his own with the computations of M. *Lieutaud*, calculated to the meridian of *Paris*, as deliver'd in his treatiſe, entituled *Connoiſſance des Temps*, and publiſh'd yearly by order of the Royal Academy. F. *Carbone* found them too wide of the truth, eſpecially as to the immerſions and emerſions of *Jupiter*'s innermoſt ſatellite: For, the obſervarions made in the Royal Obſervatory, differ from theſe ſometimes 2, ſometimes 3, and at other times even 4 minutes.

F. *Carbone* compar'd his own obſervation of the lunar eclipſe at *Lisbon*, with that made at *Paris* by M. *Maraldi*; to which he ſubjoins the compariſon of the immerſions and emerſions, obſerv'd at both places.

Tho' in the eclipſe of the moon, *Nov.* 1. 1724, the ſhadow was not well defin'd either at *Lisbon* or *Paris* (which render'd the determining theſe phaſes the more difficult) however, the greateſt part of the obſervations agrees ſo well together, that it was thought proper to compare the principal phaſes, which ſeem to have been obſerv'd with the greateſt accuracy, in order to determine the difference of the meridians of *Lisbon* and *Paris*.

h.	′	″	
1	47	45	The beginning of the eclipſe at *Lisbon*.
2	33	30	The beginning of the eclipſe at *Paris*.
	45	45	The difference of the meridians of *Lisbon* and *Paris*.
2	0	16	At *Lisbon* the ſhadow at *Ariſtarchus*.
2	46	15	At *Paris*.
	44	52	The difference.
2	11	28	At *Lisbon* the ſhadow at *Galilæus*.
2	56	20	At *Paris*.

44 51

h.	′	″	
	44	52	The difference.
2	34	37	At *Lisbon* the ſhadow at the north ſhore of *Mare Caſpium.*
3	20	30	At *Paris.*
	45	53	The difference.
2	37	17	At *Lisbon* the ſhadow at *Proclus.*
3	23	30	At *Paris.*
	46	13	The difference.
3	29	2	At *Lisbon Ariſtarchus* emerges.
4	14	30	At *Paris.*
	45	28	The difference.
3	31	34	At *Lisbon Copernicus* entirely emerged.
4	17	34	At *Paris.*
	46	16	The difference.
3	47	46	At *Lisbon Timocharis* emerged.
4	33	34	At *Paris.*
	45	48	The difference.
3	58	59	At *Lisbon Plato* entirely emerged.
4	44	23	At *Paris.*
	45	24	The difference.
4	20	36	The end of the eclipſe at *Lisbon.*
1	6	30	At *Paris.*
	45	54	The difference.

According to theſe obſervations, the duration of the eclipſe at *Lisbon* was 2h. 32′ 51″, only 9 ſeconds leſs than it was obſerv'd at *Paris*; and the difference of meridians, that reſults from the obſervations of the beginning and end of the eclipſe, is 45′ 50″; which comes very near what reſults from the compariſon of the other *maculæ*, obſerv'd at *Lisbon* and *Paris.*

At the Royal Obſervatory at *Paris* M. *Maraldi* made ſeveral obſervations on the eclipſes of *Jupiter*'s ſatellites, correſponding with thoſe ſent him from *Lisbon*; the compariſon is as follows.

	h.	′	″	
June 30. 1724	2	8	51	The immerſion at *Lisbon.*
	2	54	41	At *Paris.*
		45	50	The difference.
Sept. 2.	9	36	57	The emerſion at *Lisbon.*
	10	22	46	At *Paris.*
		45	49	The difference.

September

	h.	′	″	
September 25.	9	59	24	The emerſion at *Lisbon.*
	10	45	5	At *Paris.*
		45	44	The difference.
October 4.	6	26	44	The emerſion at *Lisbon.*
	7	11	58	At *Paris.*
		45	14	The difference.

The greateſt part of theſe obſervations agrees in making the difference of meridians between *Lisbon* and *Paris* 45′ 48″ of time, which agrees with all poſſible accuracy with that determined by the laſt obſervation of the eclipſe of the moon.

If the aforeſaid difference be true, *viz.* 45′ 48″, the difference between the meridians of *Lisbon* and *London*, will be 36′ 7″

An excretory Duct *from the* Glandula renalis. Phil. Tranſ. N° 385. p. 190.

THE celebrated anatomiſt S. *Valſalva*, already known by his noble treatiſe *de aure humanâ*, has made a conſiderable diſcovery: He has found the excretory ducts of the *glandulæ renales* or *renes ſuccenturiati*, which diſcharge themſelves into the parts of generation; that is, into the *epididymides* in men and into the *ovaria* in women. He has read a learned diſſertation, relating to this diſcovery, before the academy of ſciences in *Bologna*, in which he undertakes to prove, that thoſe *renes ſuccenturiati* are to be reckoned among the principal organs of generation.

An Account of the Currents *at the* Streights Mouth. Phil. Tranſ. N° 385. p. 191.

CAPE Spartel and *Cape Trafalgar* from the weſtern ocean are known to form the ſtreights mouth; whence a current, in the middle of the channel (which is about five leagues broad) betwixt the *Barbary* and *Spaniſh* land, runs at leaſt two miles each hour, as far as *Ceuta* point, and there the two coaſts opening about 18 leagues diſtance from each other, the current does not run above one mile an hour, and ſo continues as far as *Cape de Gat*, which is 70 leagues up the *Mediterranean*. Our mariners obſerve a current to ſet to the weſtern ſea or the great ocean from *Ceuta*, along the *Barbary* ſhore, and from *Gibraltar* along the *Spaniſh* ſhore; but that on the *Barbary* ſhore is generally their common rout, not only as being the freeſt from rocks and leſs dangerous; but becauſe the tide is much ſtronger than

than it is on the other ſide, which the ſooner helps the ſhips out of the ſtreights, which are narroweſt between the points of *Gibralter* and *Ceuta*; at which laſt place, a neck of land extends itſelf a conſiderable way into the ſea; and it is the opinion of the author and others, that whereas the current runs, as aforeſaid, two miles an hour againſt this neck of land, the water there meets with ſo violent an oppoſition in its courſe, as occaſions it to rebound with ſo much force, that part of it returns back along the ſame coaſt, and ſo out of the *Streights* mouth; which, with the ſmall tide that ſets out on the *Spaniſh* ſhore, it is believed, may exhauſt a conſiderable part of that current; which continually ſets in to the eaſtward at the rate already mentioned. What the author takes to be very remarkable is, that in 1712 M. *de l'Aigle* commander of a *French* privateer, called the Phœnix of *Marſeilles*, giving chaſe near *Ceuta* point to a *Dutch* ſhip bound for *Holland*, came up with her in the middle of the gut or *Streights*, between *Tariffa* and *Tangier* and there gave her one broadſide, which directly ſunk her; and a few days after, the ſunk ſhip with her cargo of brandy and oil, aroſe on the ſhore near *Tangier*, which is, at leaſt four leagues to the weſtward of the place where ſhe ſunk, and directly againſt the ſtrength of the current; which made many apt to think, there is a recurrency in the deep water in the middle of the gut, that ſets outwards to the grand ocean, which, the author thinks, this accident very much demonſtrates; and poſſibly a great part of the water which runs into the *Streights*, returns that way and along the two coaſts already mentioned; otherwiſe, this ſhip muſt of courſe have been driven towards *Ceuta* and ſo upwards. The water in the gut muſt be very deep, ſeveral of the commanders of our ſhips of war having attempted to ſound it with the longeſt lines they could contrive; but could never find any bottom.

Ambergris *found in* Whales; *by Dr.* Boylſton. Phil. Tranſ N° 385. p. 193.

THE moſt learned part of mankind are ſtill at a loſs about ſeveral things, even in medical uſe; and particularly ſo, in what is called *ambergris*, till our whale fiſhermen of *Nantucket* in *New England* lately made the diſcovery.

Upon cutting up a *Sperma ceti* bull whale, they accidentally found in him about 20 pound weight of that drug. After which, they and other fiſhermen became very curious in ſearching all ſuch whales they killed; and it has ſince been found in ſmaller

ſmaller quantities, in ſeveral male whales of that kind and in no other, and that ſcarcely in one of a hundred of them. They add farther, that it is contained in a *cyſtis* or bag, without any inlet or outlet thereto; and that they have ſometimes found the bag empty and yet entire.

The bag is no where to be found, but near the genital parts of the fiſh. The *ambergris* is, when firſt taken out, moiſt, and of an exceeding ſtrong and offenſive ſmell.

Obſervations on ſome of the Plants *in* New England, *with remarkable inſtances of the Nature and Power of* Vegetation; *by Mr.* Paul Dudley. Phil. Tranſ. N° 385. p. 194.

THE plants of *England*, as well thoſe of the fields and orchards, as of the garden, that have been brought over into *New England*, ſuit very well with the ſoil and grow to great perfection.

Their apples are, without doubt, as good as thoſe of *England* and much fairer to look to, and ſo are the pears; but they have not got of all the ſorts.

Their peaches do rather excel thoſe of *England*, and then they have not the trouble or expence of walls for them; for, their peach-trees are all ſtandards; and Mr. *Dudley* has had in his own garden 7 or 800 fine peaches of the *rare-ripes*, growing at a time on one tree.

The people of late years have run ſo much upon orchards, that in a village near *Boſton*, conſiſting of about 40 families, they made in the year 1721 near 3000 barrels of cyder; and in another town of 200 families, in the ſame year, Mr. *Dudley* was credibly informed, they made near 10,000 barrels. Some of their apple-trees will make 6, ſome have made 7 barrels of cyder; but this is not common; and the apples will yield from 7 to 9 buſhels for a barrel of cyder. A good apple-tree in *New England* will meaſure from 6 to 10 foot in girt. Mr. *Dudley* has ſeen a fine pear-main tree, at a foot from the ground, meaſure 10 feet and four inches round. This tree, in one year, bore 38 meaſured buſhels of as fine pear-mains, as ever he ſaw in *England*. A *Kentiſh* pippin at 3 foot from the ground, 7 foot round, and a golden roſſetin 6 foot round. The largeſt apple-tree he could find was 10 foot 6 inches round; but this was no graft.

An *orange* pear-tree grows the largeſt, and yields the faireſt fruit: He obſerved one of them near 40 foot high, that meaſured 6 foot and 6 inches in girt, a yard from the ground, and bore

bore 30 buſhels at a time; and in the year 1724 he meaſured an *orange* pear-tree, 11 inches round the bulge. He had a pear-tree, that meaſured 5 foot 6 inches round. One of Mr. *Dudley*'s neighbours had a *Bergamot* pear-tree, brought from *England* in a box, about the year 1643, that meaſured 6 foot round, and bore 22 buſhels of fine pears in one year. About 20 years before, the owner took a cyon and grafted it upon a common hedge pear; but the fruit did not prove altogether ſo good; and the rind or ſkin was thicker than that of the original.

Their peach-trees are large and fruitful, and commonly bear in three years from the ſtone. Mr. *Dudley* had one in his garden of 12 years growth, that meaſured two foot and one inch round, a yard from the ground, which two years before, bore near a buſhel of fine peaches. Their common cherries are not ſo good as the *Kentiſh* cherries of *England*, and they have no dukes or heart cherries, but in two or three gardens.

Some years before, Mr. *Dudley* meaſured a *Platanus occidentalis* or *button-wood* tree, as they are called, that was nine yards round and it held its bigneſs a great way up. This tree, when cut down, Mr. *Dudley* was informed, made 22 cord of wood. A gentleman told him, that in the foreſt he obſerved a ſtreight aſh, that grew like a pillar, of a conſiderable height and free from limbs, that meaſured 14 foot 8 inches round, near a yard from the ground; and Mr. *Dudley* ſaw a ſaſſafras tree, that meaſured five foot three inches round. Among their trees of quick and eaſy growth, the abovementioned *button-wood* and the *locuſt* tree, are the moſt remarkable: As to the latter, according to the deſcription Mr. *Moore*, while in *New England*, gave Mr. *Dudley* of the *manna* tree, their *locuſt* tree may be called the *American manna*. A ſeed of it, blown off from the tree into Mr. *Dudley*'s garden, took root of itſelf; and in leſs than two years was got above ſix foot high, and as big about, as a common walking cane. He ſeveral times propagated the *platanus*, by cutting off ſticks of five or ſix foot long, and ſetting them a foot deep in the ground in the ſpring of the year, when the ſeaſon is wet; they thrive beſt in a moiſt ſoil.

An onion, ſet out for ſeed, will riſe to four foot nine inches in height; a parſnip to eight foot; red orrice to nine foot and white orrice to eight. In the paſture-grounds Mr. *Dudley* meaſured ſeed mullen nine foot two inches in height; and one of the common thiſtles upwards of eight foot.

Among the remarkable inſtances of the power of vegetation Mr. *Dudley* had a well atteſted account from Mr. *Edwards* of

Windſor

Windſor of a pompion ſeed, as follows; that in 1699 a ſingle ſeed was accidentally dropped in a ſmall paſture, where cattle had been foddered for ſome time. This ſingle ſeed took root of itſelf, and without any manner of care the vine run along over ſeveral fences, and ſpread over a large piece of ground far and wide, and continued its progreſs till the froſt came and killed it: This ſeed had no more than one ſtalk, but a very large one; for, it meaſured eight inches round: From this ſingle vine they gathered 260 pompions; and one with another, as big as a half peck; enough in the whole to fill a large tumbrel, beſides a conſiderable number of ſmall and unripe pompions, they made no account of. There is an account in the *Philoſophical Tranſactions* of a ſingle plant of barley, that by ſteeping and watering it with ſalt-petre, diſſolved in water, produced 249 ſtalks and 18,000 grains; but then there was art and even force in that caſe: Whereas, in this, there was nothing but pure nature and accident.

The *Indian* corn is the moſt prolific grain they have, and commonly produces 1200, and often 2000 grains from one ſingle grain; but the faireſt computation is thus; ſix quarts of this grain will plant an acre of ground; and it is not unuſual for an acre of good ground to produce 50 buſhels of corn.

Indian corn is of ſeveral colours, as blue, white, red and yellow; and if they are planted ſeparately, ſo that no other ſort be near them, they will retain their own colour. But if in the ſame field you plant the blue corn in one row of hills (as they are called) and the white or yellow in the next row, they will mix and interchange colours; that is, ſome of the ears of corn, in the blue corn rows, ſhall be white or yellow; and ſome again, in the white or yellow rows, ſhall be blue. The hills of *Indian* corn are generally about four foot aſunder and ſo continued in a ſtreight line, as far as the field will allow; and then a ſecond line or row of hills, and ſo on; and yet this mixing and interchanging of colours has been obſerved, when the diſtance between the rows of hills has been ſeveral yards: And Mr. *Mayhew* of *Martha's Vineyard*, an iſland in the province of *New England*, aſſured Mr. *Dudley*, that the blue corn has thus communicated or interchanged, even at the diſtance of four or five rods; and particularly in one place, where there was a broad ditch of water between them. Some of our people, but eſpecially the *Aborigines*, have been of opinion, that this commixture and interchange of colours, was owing to the roots and ſmall fibres, reaching to and communicating with each

each other; but this muſt certainly be a miſtake, conſidering the great diſtance; eſpecially at ſome times and a-croſs a canal of water; for, the ſmalleſt fibres of the roots of the *Indian* corn cannot extend above four or five foot: Mr. *Dudley* is, therefore, of opinion, that the *ſtamina* or principles of this ſurpriſing copulation or commixture of colours are conveyed by the wind; and that the ſeaſon of it is, when the corn is in the earing, and while the milk is in the grain: For, at that time, the corn is in a ſort of eſtuation and emits a ſtrong ſcent. One thing, which confirms the air being the medium of this communication of colours in the corn, is an obſervation of one of Mr. *Dudley*'s neighbours, *viz.* that a cloſe, high board fence, between two fields of corn, that were of a different colour, entirely prevented any mixture or alteration of colour, from that they were planted with.

An apple-tree in the town, belonging to Mr. *Dudley*, bears a conſiderable quantity of apples; eſpecially every other year, tho' it never had a bloſſom: For three years, he went in the proper ſeaſon and obſerved it; and when all the reſt of the orchard was in the bloom, this tree had not one ſingle bloſſom: Not contented with once going, he went again and again, till he found the young apples perfectly formed. In 1723 he went early, not knowing but it might blow ſooner than the other trees; but he found no bloſſoms; and the owner with ſeveral of his neighbours, aſſured Mr. *Dudley* they had known the tree upwards of 40 years, and that it never had any bloſſom. Mr. *Dudley* opened ſeveral of the apples and obſerved but very few ſeeds in them; and ſome of them lodged ſingle in the ſide of the apple. This tree was no graft, and the fruit but ordinary for taſte: He could not perceive, by his obſervation, but that in all other reſpects, it fructified like other apple-trees.

A Diſſertation on the Figure of the Earth; *by Dr.* Deſaguliers. Phil. Tranſ. N° 386. p. 201.

THAT the earth is of a ſpherical figure, or nearly ſuch, has been proved ſo often and by ſo many unanſwerable arguments, that it would be tedious to repeat them. But as a little variation from a true ſphere (beſides the irregularity of high hills and deep valleys) does not hinder us from calling the earth a globe; ſo to determine what that variation may be, ſince modern philoſophers are divided about it, may not be an ungrateful ſubject.

M. *Caſſini*

M. *Caſſini* affirms 'that the earth is an oblong ſpheroid, higher at the poles than at the equator, making the axis longer than the equatorial diameter about 13 *French* leagues; which he deduces from comparing his father's meaſures of the meridian from *Paris* to the *Pyrenæan* mountains, with thoſe of M. *Picard*; of which an account may be ſeen in the *Memoirs of the Royal Academy* for the year 1713. But having afterwards continued the meridian, which is drawn thro' *France*, from *Paris* to *Dunkerque*, he ſtill draws conſequences to prove the earth an oblong ſpheroid; but then he makes the axis exceed the equatorial diameter 34 leagues.

'Sir *Iſaac Newton* makes the earth higher at the equator; and conſequently, flatted towards the poles, reckoning its equatorial diameter 34 *Engliſh* miles longer than the axis; which he proves from the principles of gravity, and the centrifugal force, that ariſes from the diurnal rotation of the earth; and to confirm this he mentions ſeveral experiments on pendulums, which have been made ſhorter near the equator, than in greater latitudes, to ſwing ſeconds.

Theſe are the two opinions which have divided philoſophers, nd which Dr. *Deſaguliers* propoſes to examine.

M. *Caſſini* taking the meaſures abovementioned to be exact nough, not only to determine the magnitude of a degree of the arth, correſponding with a degree of a great circle of the hea-ens; but alſo to ſhew the difference in the degrees of the earth reckoning thoſe meaſured in the ſouth of *France* to exceed thoſe owards the north, by a certain number of toiſes and feet) demonſtrates, that if the degrees of the earth are longer towards he equator than the poles, the plane of the meridian muſt be n ellipſe, whoſe long axis is that of the earth. Here follows is firſt demonſtration; vide the *French Memoirs* for the year 1713.

'Let BDCR (Fig. 13. Plate XII.) be an ellipſis repreſenting a meridian of the earth, whoſe poles B and C are at the extremities of the great axis BC, and whoſe *foci* E and F are taken at pleaſure. Now to divide this ellipſe into degrees, that is, to find ſeveral points HIV, ſuch, that the diſtance of every one of them, from the pole to the zenith, ſhall be of any given number of degrees.

'*Demonſtration.* From E one of the *foci* of the ellipſis, draw the line ET; ſo that it may form, with the axis BC, the angle BET, equal to the diſtance given from the pole to

'the

'the zenith: From the other *focus* F, with the distance B(
'equal to the axis, draw an arch to cut the line E T at T;
'say, that the line F T, drawn from the point T to the *foci*
'F, will cut the ellipsis at the point H; which point is such
'that the distance of the pole, from its zenith, contains th
'given number of degrees.

'From the point H, raise H Z, perpendicular to the ellip
'sis, which will pass thro' the zenith Z; and being produce
'inwards, will meet the axis of the earth at O; and (by th
'property of the ellipsis) divide the angle E H F into tw
'equal parts: From the point H, likewise draw H P, paralle
'to the axis B C, and directed to the pole P, supposed at a
'infinite distance: The angle P H Z or P O Z, measures th
'distance from the pole to the zenith, of an inhabitant dwel
'ling upon the earth at the point H. F T is equal to the axi
'B C, by construction; but by the property of the ellipsis, B(
'is equal to E H plus H F, taking away from both F H, whic
'is common, E H will remain equal to H T; the angles E T
'H, T E H, will, therefore, be equal, and consequently, each
'of them will be half of the external angle E H F: But th
'angle E H O is likewise equal half the angle E H F; there
'fore, the angles T E H, E H O, will be equal to each other
'and consequently, the lines E T and H O will be parallel t
'each other; and the angle P O Z, which measures the dis
'tance, from the pole to the zenith of the point H, will be
'equal to the angle B E T, which was, by construction, taker
'equal to the given distance of the pole from the zenith;
'Q. E. D.

'Now, if the proportion of the longest diameter of the el
'lipsis B C to E F, the distance of the *foci*, be taken at plea
'sure, one may, by calculation, find all the points of the
'ellipsis as H, to determine the degrees by this analogy as F T
'or B C: is to E F:: So is the sine of the angle P E T (the
'given distance from the pole to the zenith): To the sine of
'the angle E T F, or T E H; whose quantity will consequently
'be known. This angle T E H being added to the angle P E
'F, the given distance, from the pole to the zenith, of the
'point H, will give the quantity of the angle B E H, which
'a line drawn from the *focus* to H, the point required, makes
'with the axis of the ellipsis.

'Then, in the triangle E H F, whose side E F is known,
'as well as the angle E H F, which is double the angle T E H,

'and

and the angle FEH ſupplement of the angle BEH; one ſhall have the length of the ſide EH, known in parts of the axis BC.

'After the ſame manner may be found the angles BEI, BEV, &c. and the length of the lines EI, EV, to determine the diſtance, from the pole to the zenith, of all the degrees of the circumference of the earth; and in the rectilinear triangles HEI, IEV, whoſe ſides HE, EI, EV, are known, as well as the angles, comprehended between the ſides HE, EI, EV, which are the differences of the angles BEH, BEI, BEV, determined above; one ſhall find the length of the chords HI, IV, comprehended between each degree.'

M. *Caſſini* in the *Memoirs* for the year 1718, repeats the ſame demonſtration; only that, before it, he ſhews, that if ſeveral points be taken upon a terreſtrial meridian, on the ſurface of an elliptic earth, as GHIK, (Fig. 14.) in ſuch manner that their reſpective zeniths ZLMN, are diſtant from each other, an equal number of degrees, meaſured in a celeſtial meridian; the lines ZG, LH, MI, NK (which are perpendicular to the ellipſis) being produced, will meet in the points O, R and S, forming equal angles: But as thoſe angular points are not equally diſtant from the curve of the ellipſis, that elliptic arch muſt be the longeſt, whoſe angular point is fartheſt off. Now, by the former demonſtrations it appears, that thoſe arches, which are taken neareſt to the leſſer axis, will have their angular points farther removed, &c.

If M. *Caſſini*'s meaſures of terreſtrial degrees, decreaſing from the equator towards the pole, were grounded on obſervations liable to no error, he would have fully proved his figure of the earth: But ſince thoſe meaſures (however accurately taken) are not built upon a mathematical certainty, his premiſes may be called in queſtion; and his concluſion, tho' mathematically drawn from theſe premiſes, is only probable.

Now therefore, if Dr. *Deſaguliers* can ſhew from undoubted phænomena, that his concluſion will lead to an abſurdity, his meaſures muſt be falſe; tho' his reaſoning from them be juſt. This the Dr. endeavours to do firſt, which will diſprove his figure of the earth; and afterwards to point out ſome of the errors, which he ſuppoſes to have occaſion'd the miſtake in the meaſures.

M. *Caſſini*,

M. *Caſſini*, as well as the *Engliſh* aſtronomers, believes that the earth makes one revolution about its axis, once in $23^h\ 56'$; becauſe in that time, the plane of any meridian returns to the ſame fixed ſtar, from which it had departed.

Let H be taken in any parallel of Lat. as for example, in the Lat. of 51° 46′, a plumb line L H, will be perpendicular to the curve B H at H, and produced paſs thro' the zenith of the point H, if the earth had no diurnal rotation; but ſince the earth moves round its axis, all bodies upon its ſurface, endeavour to fly from the axis of their motion with a force proportionable to their diſtance from it, in a direction along the plane of that parallel, in which they are. Let that force (explained by M. *Huygens* and called a centrifugal force) be repreſented by the line H *l*, or its equal and parallel L *h*; now a plummet placed at L, if the earth ſtood ſtill, would deſcend in the line L H; but as it is at the ſame time acted upon by the force H *l* in the direction L *h*, it will move in the direction L *t*, diagonal of the parallelogram H *l*, according to the known laws of mechanics; and the plumb line L H, inſtead of being perpendicular to the curve at H, will in Lat. 51° 46′ form an angle of 5′ with H L. This angle will be leſs towards the poles, till it quite vaniſh at the pole, as it alſo does at the equator. Now ſince there is no ſuch angle obſerved; but in all water levels we find the plumb-line, always perpendicular to the line of level, the ſurface of the earth muſt be depreſſed towards G, and riſe farther from the axis towards I, in order to become perpendicular (that is, to have its tangent perpendicular) to the line L *l*, in which we have ſhewn the plumb-line muſt deſcend.

If there be any body ſo fond of M. *Caſſini*'s hypotheſis, as to deny the diurnal motion of the earth, for the ſake of it, the Dr. hopes they will be convinced, when he ſhews the meaſures, upon which it is fonnded, to be inſufficient for determining the different lengths of the degrees of a terreſtrial meridian.

But here the Dr. would not be thought to endeavour to leſſen the commendation due to the Gentlemen of the *Royal Academy of Sciences*, for carrying on a meridian the whole length of *France*, from *Dunkerque* thro' the *Royal Obſervatory* at *Paris*, quite to the *Pyrenæan* mountains on the borders of *Spain*. Aſtronomy and geography are, doubtleſs, much indebted to the encouragement, given by the *French* government, and to the care of their mathematicians, who have omitted

omitted no proper method for drawing their meridian, and correcting it, as they went on. So many obſervations of the riſing and ſetting ſun, ſo many equal altitudes of the ſame ſtars, accurately taken, ſo many digreſſions of ſtars, ſo many other obſervations, made with the teleſcope and good pendulum-clocks, &c. all compared together, for the true ſettling of the direct way of this famous meridian, leave no doubt, but that it is as perfect, as the nature of the thing is capable of. And certainly, by the help of this line, and the ſeveral triangles made uſe of for carrying it on, a better map of *France* is made, than has ever been of any country before: Nay, beſides, the Dr. thinks, we may, at a medium, very well receive their number of 57,060 or 57,061 toiſes, for the meaſure of a degree of a meridian of the earth, one with another. But to ſay, that thoſe gentlemen could obſerve the latitude ſo nicely, as to find a difference in the length of the terreſtrial degrees, and that only of 11 or 12 toiſes (when they made it the leaſt) or of 31 toiſes (when they made it the moſt) is attributing to them an exactneſs, ſo far beyond the nature of the inſtruments, which they made uſe of, that it would be rather a diſpraiſe than commendation to inſiſt upon it.

For, in the firſt place, the inſtrument, with which they took obſervations for the latitude, at the two ends of their meridian, was a ten foot ſector (which was worſe than that, which M. *Picard* had made uſe of before; becauſe the teleſcope of his ſector was of 10 foot; whereas M. *Caſſini*'s was but of three foot, tho' applied to the 10 foot ſector) where the two hundredth part of an inch anſwers to 8 ſeconds of a degree. Now the two hundredth part of an inch, being one of the leaſt viſible parts, that we can ſee in a divided line, they could not take an angle nearer than that; nay, their inſtrument, according to their own deſcription of it, was divided but to every 20 ſeconds. Now they allow, that 16 toiſes upon the ſurface of the earth, anſwer to 1 ſecond in the heavens; and they do not pretend to have taken an obſervation nearer than to about 3 ſeconds; which, therefore, cannot determine a difference leſs than 48 toiſes; whereas the degrees are only ſuppoſed to decreaſe, at moſt, 31 toiſes each, from *Collioure* to *Dunkerque*. But an error of 8 ſeconds would make a difference of 128 toiſes, on the ſurface of the earth, above 10 times greater than the difference of degrees in the firſt ſuppoſition, and 4 times greater than that diffe-

difference in the laſt. Beſides, the Lat. was not obſerv'd in the intermediate places between *Paris* and *Collioure*, with the above-mentioned inſtrument of 10 foot radius; but they made uſe of a quadrant, whoſe radius was only 39 inches, and ſometimes an octant of three foot radius: Nay, they ſay themſelves, in their account, that it is not the obſervations, made at the ends of the meridian, that we are to deduce the difference of the length of a degree from; but the altitudes, taken at ſeveral places between the extremes; and if we grant, that they can take an angle very well, to four or five ſeconds, with the large inſtrument, they cannot come nearer than 12 or 15 ſeconds, with the quadrant or octant, which we muſt depend upon for the difference of the meaſure of degrees: So that upon the whole, we are to determine a length of 31 toiſes, by an inſtrument, which is liable to error upwards of 200 toiſes.

If any conſequences of this kind could be drawn from actual meaſuring, a degree of Lat. ſhould be meaſur'd at the equator, and a degree of Long. likewiſe; and a degree very northerly; as for example, a whole degree might be actually meaſur'd upon the *Baltic* ſea, when frozen, in the Lat. of 60 degrees. There, according to M. *Caſſini*'s laſt ſuppoſition, a degree would be of 56,653 toiſes; whereas, at the equator, it would be of 58019 toiſes; the difference being 1364 toiſes, that is, about the two and fortieth part of a degree, which muſt be ſenſible; and the degree of Long. would likewiſe be, according to him, of 56,87 toiſes, by 1202, or the forty-eight part leſs, than a degree of Lat. at the ſame place.

But here it may be objected, that tho' the Lat. was not taken with the ten foot ſector, in the intermediate places between *Paris* and *Collioure*; yet the Lat. was taken with that inſtrument at *Dunkerque*, *Paris* and *Collioure*; and therefore, the ſouthern part of the meridian, containing 6° 18′ 56″, may be compar'd with the northern part, which contains 2° 12′ 16″; and that the former ſeems to contain more toiſes, in proportion to the difference of Lat. at its extremities, than the latter. To this may be anſwered, that, even in this caſe, the obſervations made cannot be accurate enough to determine the difference of the length of degrees: But there is another error, which might conſiderably miſlead the *French* gentlemen, and make the degrees appear longer in the ſouth of *France*, that is, the error in taking the true height of ſeveral mountains in *Auvergne*, *Languedoc*, and among the *Pyrenees*. For, if they have allow'd too much for

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the refraction of the air (which, by the obſervations of travellers, is greater towards the northern regions, and diminiſhes as we go ſouthward) the heights of thoſe mountains will be taken too little, and their baſes, conſequently, longer, which will make the degrees appear larger than they are.

For example, let ABCD (Fig. 15.) be a mountain, as that of *Rodez* in the Lat. of 44° 21′, whoſe height BD is 300 toiſes, and whoſe ſides AB and BC (ſuppos'd to form an angle of 26° 33′ with the horizon) are found by trigonometry to be of 670,8 toiſes each; if by a miſtake, in taking the height, it be ſuppos'd only equal to ED or 257 toiſes; then the lines AB and BC will become EF and EG; ſo that the baſe AC, which before was of 1200 toiſes, will become equal to FG, which will appear to be 1279,6 toiſes (by *Euch.* 47. 1.) Now one ſuch miſtake in one degree will give a difference above twice as great as the ſuppos'd difference of degrees in that Lat. which they make of 31 toiſes: And that there was a miſtake of this kind in taking the height of that mountain, the Dr. will ſhew anon.

The vapours that generally float in the air about the top of high hills, make it ſo difficult to take their heights exactly, that experiments, made with the barometer, will, by obſerving the fall of the mercury, ſhew the height nearer than any thing elſe we know of. There were, it is true, ſeveral experiments made with the barometer (vide Memoirs of the Royal Academy for the year 1718. c. 10.) where the differences of the height of the mercury, from the heights at which it ſtood at the Royal Obſervatory, are ſaid to anſwer to ſo many toiſes: But of nine obſervations mentioned by M. *Caſſini*, there are not two, where the number of toiſes, ſaid to correſpond to the heights of the barometer, do agree together.

The firſt experiment of the barometer, there mentioned, made at *Collioure*, was this; 'at the height of 11 toiſes and '½ above the ſea, the barometer was ſet up, and the mercury 'ſtood three lines and ⅓ higher than at the Royal Obſerva-'tory (in the Tower of the eaſtern hall) at the ſame time; 'and therefore, ſince that tower is 44 toiſes higher than the 'ſea, three lines and a third of mercury muſt anſwer to 32 'toiſes and ½'.

Now, reducing theſe toiſes to feet, and dividing by 3 and ⅓, it will appear, that a height of 58,5 feet will anſwer to the fall of one line of mercury in the barometer: Let this be taken

taken as the ſtandard, and the other obſervations compar'd with it. This may be done by the following table, where the firſt column ſhews the place, where the obſervation was made; the ſecond, the fall or riſe of mercury at each place, expreſs'd in lines, or twelfth parts of a *French* inch; the third, the heights or depths anſwering to thoſe lines of mercury, which, in the Memoirs are given in toiſes, but here reduced to feet; the fourth, the number of feet anſwering to one line of mercury in each obſervation, which is the quotient of the feet in the third column, divided by the number of lines in the ſecond column.

Obſervations on the barometer, made at	Lines of mercury	Said to correſpond with feet	The fall of one line of mercury correſ. with feet
I *Collioure.*	$3\frac{1}{3}$	195	58, 5
II The tower of *Maſſane*	31	2382	76, 8
III *Bugarac*	42	3636	86, 5
IV *Rupeyroux*	30	2181	72, 7
V *Rodez*	24	1647	68, 6
VI *Rodez*	20	1425	71, 25
VII *Courelande*	54	4812	89, 1
VIII *Coſte*	54	4890	92, 4
IX *Clermont*	3	200	66, 6

A ſight of this table will convince any one, that theſe obſervations are not to be depended on, for determining the height of the mountains in the ſouth of *France*: For, the differences are not ſmall, ſuch as might happen in making the experiments; but ſuch, as render the obſervations, uſeleſs for the abovementioned purpoſes. For example, the firſt and ſeventh differ almoſt $\frac{1}{3}$; and if 58, 5 feet were allow'd for the fall of one line of mercury in the ſeventh obſervation, inſtead of 944 feet, then the mountain of *Coſte* would be but 3085 feet, inſtead of 4890. Nay, upon examining the Memoirs, the Dr. finds, that in ſeveral obſervations, the number of toiſes ſaid to correſpond to a certain height of mercury, are only anſwerable to the height of the mountain above the level of the ſea, found by trigonometry, from which the height of the Royal Obſervatory above the ſea is ſubſtracted; tho' by the manner of expreſſion a curſory reader would imagine, that the number of toiſes nam'd, was always proportionable to the fall of the mercury, and think all the experiments and obſervations

vations very accurately made, when they ſeem to agree ſo well in every reſpect.

Now after all, the Dr. does not queſtion, but that the height of the barometer might be, as it is ſet down in the Memoirs, and well enough obſerved. But it was wrong to compare the height of the mercury in the ſouth of *France*, with the height the mercury was at, in the barometer of the Royal Obſervatory at the ſame time: For, at that great diſtance and difference of Lat. the weather (and conſequently, the preſſure of the atmoſphere and height of the barometer at the ſame level) might vary very much.

Even when there is fair weather all over *France*, it does not follow, that the barometer ſhall ſtand at the ſame height. Let us ſuppoſe, for example, that a north wind blows; wherever the air is check'd by a chain of mountains that run eaſt and weſt, it will be accumulated over thoſe mountains, and conſequently, preſs more, as its columns are higher; which will make the mercury riſe higher than it would do with the ſame wind, if there were no mountains, or if they ran north and ſouth.

The way to have made the experiments with the barometer exactly, would have been to have obſerved the height of the mercury, at the top and at the bottom of the mountain, and that with a tube of a pretty large bore, with a proportionably large ciſtern for the ſtagnant mercury; becauſe, in a ſmall tube, the mercury will often adhere to the ſides, and riſe irregularly, as it will alſo in inclin'd barometers: Simple barometers are the beſt, and a magnifying glaſs may be made uſe of to obſerve ſmall riſes or falls, having two fine and well made indices to the tube.

Dr. *Halley* has, in a former Tranſaction, exhibited the falls of mercury in the barometer, correſponding with the heights to which the barometer muſt be carried to produce thoſe falls. He makes the firſt tenth part of an inch in the fall of mercury to correſpond to a height of 90 foot; the next tenth, to a height ſomewhat greater; and ſo in proportion, as the air diminiſhes in denſity, according as we riſe in height. He has built the proportion of the firſt tenth of the mercury's fall upon the compariſon of the different ſpecific gravities of air and mercury; and taking mercury to be 13 ½ times heavier than water; and water in cold weather to be 800 times heavier than air; it follows that 13, 5×800 will give 10800; which number, if taken in feet and divided by

120 (the number of the tenths of an inch in a foot) we ſhall have 90 feet correſponding to the tenth part of an inch, and 75 feet to a line or the twelfth part of an inch.

Now, as very few mountains in the world are three miles high; and generally ſpeaking, thoſe we look upon as high (excepting the *Andes* and ſome others in *America*) are not much above a mile; we may, for finding the height of mountains, take a fix'd number of feet in altitude to correſpond to every tenth or twelfth part of an inch in the fall of the mercury; becauſe 90 feet only are taken by Dr. *Halley* for the firſt tenth, and greater heights for other tenths, encreaſing with the fall of the mercury; Dr. *Deſaguliers* therefore would propoſe another for a round number, namely 96 feet for every tenth, and 80 feet for every twelfth of an inch, very near the number he found by calculation, as follows.

Fine mercury (ſuch as is made uſe of in barometers) is generally ſpeaking 13 $\frac{2}{3}$ times heavier than water; and he found ſome brought from the *Eaſt-Indies* to be 14 times heavier. He found air in ſummer to be near 900 times lighter than water; and 800 times lighter in winter; he therefore takes 850 at a medium. Now $850 \times 13\frac{2}{3} = 11606,6$, which divided by 120, gives 96,7 feet, for one tenth of an inch of mercury, or 80,5 feet for one twelfth of an inch. This number, taken invariable, will, in taking the height of ſeveral hills, agree pretty well with the numbers that come out, when Dr. *Halley*'s table is made uſe of; and with the experiment made by Mr. *Caſwell*, who, having taken the height of *Snowdon-hill* in *Caernarvonſhire* very accurately, and finding it to be 3720 feet above the level of the ſea, tried how much lower the mercury would ſtand in the barometer upon that hill, than at the level of the ſea, and he obſerv'd it to ſubſide 3,9 inches. The Dr. is ſenſible, that it will be alledged, that the air will be denſer than he may imagined on the top of high hills; becauſe of the great cold, ſince they are generally cover'd with ſnow: But then we are to conſider, that when we are got above a mile higher than the level of the ſea, the incumbent atmoſphere has loſt almoſt a fifth part of its weight; and therefore, the air, at the top of the hill, being ſo much leſs preſs'd, will, notwithſtanding the intenſe cold, be more rarified than at the bottom of the hill.

Now, if we go back to the obſervations of the barometer, made by the gentlemen that drew the meridian in *France*, we ſhall find, that on the mountain of *Rodez*, in Lat. 44° 21. the

the barometer fell 24 lines below the level of that in the Obſervatory, and they allow'd only 274 toiſes and ½ to correſpond to that fall: Whereas, according to Dr. *Halley*'s proportion of a tenth of an inch for 90 feet, they ſhould have taken 300 toiſes; and tho' the hypothenuſes A B and B C (Fig. 15.) were taken longer than the bare declivity of the mountain, (which would make the error leſs than the 79 toiſes above-mentioned) yet if Dr. *Deſaguliers*'s proportion be made uſe of, *viz.* 80 feet for each line of mercury, it will make the mountain 320 toiſes, which being higher, will, therefore, ſhew the baſe to be yet ſhorter; and conſequently, the error at that rate will be greater.

This error (and ſuch like, if any more were made) will increaſe the meaſure of the 44th degree of Lat. on the earth; and by obſerving what was done in the next degree, we ſhall find, that that degree was taken too ſhort. In the Lat. of 45° 38′ the mountain of *Coſte* is made 815 toiſes high; whereas the 54 lines of the falling mercury in the barometer, ſaid to anſwer to that height, will give but 705, 6 toiſes (which we will call 705, 5) even according to the Dr's computation of 80 feet to a line, which is the greateſt allowance. If we ſuppoſe this mountain to riſe in an angle of 26° 33′, as we did that of *Rodez*, the ſides of the mountain, or hypothenuſes A B and B C (Fig. 16.) will be each equal to 1577, 54 toiſes, and the whole baſe A C to 2822 toiſes. Now, when the height of this mountain is call'd 815 toiſes, the baſe A D or D C (by *Euch.* 47. 1.) becomes only equal to F D or D G = 1350, 7; and its double or F G, the whole baſe, will be but 2701, 4 toiſes, leſs than the former by 120, 6 toiſes.

This error is ſo great (ſo much more than the difference of 31 toiſes for a degree) that tho' the Dr. ſuppoſed the lines found by trigonometry, which terminate at the top of the mountain, to be much longer than the hypothenuſe A B, yet there will be error enough to make the 45th degree of Lat. appear much ſhorter than it really is, ſuppoſing (becauſe of the length of the lines A B, or the great diſtance from which the mountain might be obſerv'd) that theſe errors were four times leſs than the Dr. has made them; yet at that rate, one muſt add near 20 toiſes to the 44th degree of Lat. and take away above 30 from the 45th degree, which will make the 44th of 57080 toiſes, and the 45th of only 57050; and this will give a difference of 50 toiſes: So that if an angle can be taken to two or three ſeconds, to which 32 or 48 toiſes, are

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ſaid to anſwer upon the ſurface of the earth, ſuch a difference might be viſible; and much more ſo, if other errors of the ſame kind ſhould happen to have been made the ſame way; or if thoſe errors were nearer his firſt ſuppoſition than this laſt. Nay, tho' the 45th degree of latitude may be 13 toiſes larger than the 44th, it might by this means appear to be conſiderably leſs.

Such a miſtake might be the occaſion of making the hypotheſis of the earth being an oblong ſpheroid; eſpecially, becauſe in this hypotheſis, the degrees differ moſt in length from each other about the 45th degree; and when once an hypotheſis is ſet on foot, we are too apt to draw in circumſtances to confirm it; tho' perhaps, when examined impartially, they may rather weaken than ſtrengthen our hypotheſis; otherwiſe the author of the hiſtory of the *Royal Academy* for the year 1713, would not have alledged, that M. *Caſſini* the elder had obſerved *Jupiter* to be oval, as a proof of young M. *Caſſini*'s hypotheſis; becauſe *Jupiter* is oval the other way, that is, an oblate ſpheroid, flatted at the poles, as the ſaid M. *Caſſini* the elder gave the proportion of the axis to the equatorial diameter to be as 15 to 16: And Mr. *Pound*, with a teleſcope of 123 foot *focus* and an excellent micrometer, has given thoſe proportions, as 11 to 12. If a proof is to be drawn from analogy, or what is obſerved in other planets, this muſt deſtroy M. *Caſſini*'s hypotheſis and confirm Sir *Iſaac Newton*'s.

The opinion of Dr. *Burnet*, quoted in the *Memoirs*, for the year 1713, is but a very weak argument, in favour of M. *Caſſini*'s hypotheſis, on account of the reaſon Dr. *Burnet* gave, to prove the earth higher at the poles than at the equator: For, he ſays, 'that the velocity of the parts of the earth, in its diurnal rotation, being greater at the equator than towards the poles, all the water muſt be driven towards the equatorial regions; from whence being repelled by the reſiſtance of the air, it muſt run off again towards the poles; and ſo the figure of the water was lengthened out into an oblong ſpheroid; and conſequently, the cruſt of the earth over it did put on the ſame figure, *&c.*'

But why the air ſhould reſiſt more towards the equator than the poles, Dr. *Burnet* did not give any reaſon; and if it had been ſo, the ſame force that drove the water towards the equator, muſt have kept it there. The Dr. in the latter part of his aſſertion, forgot what he had ſaid in the former: For, the water could not run off towards the poles, whilſt the earth continued

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its rotation with the ſame velocity; for, if he had conſidered, he would have found his argument in other words to run thus; 'becauſe bodies, that move in a circle, always endeavour to recede from the axis of their motion; therefore, the water, by that endeavour, comes nearer to the axis of its motion;' which is abſurd. But Dr. *Burnet* afterwards altered his opinion, as Dr. *Deſaguliers* was credibly informed.

Dr. *Deſaguliers* having thus given his reaſons for diſapproving of M. *Caſſini*'s opinion, concerning the figure of the earth; comes now to conſider Sir *Iſaac Newton*'s, who makes it higher at the equator, than at the poles; but before he enters upon it, he quotes a paragraph out of the *hiſtory of the Royal Academy* for the year 1713: 'Reaſonings, *ſays the Author*, drawn 'from the different lengths of a *pendulum* in different climates, 'or from the inequality of the centrifugal force, ariſing from 'the diurnal motion of the earth, are, perhaps, too nice to 'produce a certain conviction; nay, perhaps, we are not well 'enough aſſured of the principles; and the conſequences may 'ſometimes be different: And therefore, it is evident, that the 'beſt way in this enquiry, is only (as M. *Caſſini* does) to make 'uſe of unqueſtioned obſervations, which ſerve directly to decide the matter.'

That M. *Caſſini* has not made uſe of unqueſtioned obſervations; and that the meaſures he mentions are not able to decide the queſtion, appears from what has been already ſaid. We muſt, therefore, ſhew, whether the principles, from which Sir *Iſaac Newton* has deduced his figure of the earth, be fully proved or not; whether the concluſion drawn from them be plain and evident; and whether the experiments on pendulums that confirm the theory, be eaſy to be made and ſuch as may be depended on.

Tho' Sir *Iſaac Newton*, in his *Principia*, has not endeavoured to give the cauſe of gravity, or to determine, whether it be owing to an impulſe or not; yet he has ſhewn what its effects and laws are, from plain experiments made by others and himſelf. From the laws of gravity and from the obſervation of a comet (vide *Princip. lib.* 3. *Prop.* 12, 13 and 42) he has deduced the annual motion of the earth; and it muſt have a diurnal motion, if it have an annual one; otherwiſe it will not agree with the phænomena. The laws of the centrifugal force, or that force by which a body, whirl'd round in any circle, endeavours to recede from the center of its motion, have been demonſtrated by M. *Huygens*.

These are the principles from which Sir *Isaac Newton* draws his conclusion; and tho' some persons, that will not be at the pains to examine them, may deny them in the lump; yet no body has yet been able to shew any flaw in the demonstrations that relate to them.

The Dissection of an Ostrich; *by Mr.* John Ranby. Phil. Trans. N° 386. p. 223.

MR. *Ranby*, having separated the muscles of the *abdomen*, which in this subject were only two oblique pair, observed between their tendons, which were very strong, and the *peritonæum*, which was exceeding thin, a thick layer of serous fat, whose office, considering the smallness of the *epiploon* and the few adipose vesicles of the mesentery, with the thinness of the *peritonæum*, might, probably, be to supply the place both of *epiploon* and mesentery in other animals, and lubricate the intestines.

In this subject there were two distinct ventricles, contrary to the observation of the *Royal Academy* at *Paris*; the first, and in its natural situation, the lower, which the Members of the said *Academy*, call the craw, and suppose to be only a dilatation of the *oesophagus*, was considerably larger than the second and uppermost muscular one; besides that it had strong muscular fibres, both circular and longitudinal: The *duodenum* comes immediately out of the second ventricle.

Both ventricles were distended beyond their usual form and filled up with so large a quantity of food of different kinds, as stones, bones, sticks, grain and other food, that it was almost impossible for them to perform their office of digestion, which, very likely, was one of the chief causes of the animal's sickness and death; and indeed, the contents of both seemed to have undergone but very little or no alteration. The *epiploon* partly covered the first ventricle; but was no ways proportionable to the size of the animal.

The spleen was fastened by a membrane to the right side of the second ventricle; and was very small, considering the size of the animal.

The glands of the mesentery were hardly visible; but the veins and arteries were very discernible.

The *cæcum*'s in this subject were near three foot in length, and one inch eight lines in diameter; they were fastened to the *ileum* and not to the *colon*, as the Gentlemen of the *Royal Academy* assert.

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To their deſcription of the kidneys Mr. *Ranby* has nothing to add, only that the two *ureters* lay upon their ſurface, as they do in other birds; and by their different branches, coming from all the parts of the kidney, of which the ſuperior was very diſcernible, entered the kidney about its middle, and formed there a very large *pelvis*.

The liver was in the ſame cavity with the heart, one half of which it nearly covered; it had no gall-bladder, and but one *ductus bilarius* inſerted into the *duodenum*, about two inches below the *pylorus*, which ſeemed to have an immediate communication with the *vena portæ*; becauſe by blowing into it, this latter was alſo diſtended. The heart and liver were ſeparated from the inteſtines by a membranous diaphragm.

Both the heart and liver were ſuſpended by one common *mediaſtinum*, by the help of its ſeveral membranes, and 8 ſtrong muſcles on each ſide, ariſing from the upper part of the ribs, going from thence over the lungs, and ending in a very ſtrong tendinous membrane, which is inſerted into the *ſpina dorſi*.

The liquor, contained in the *pericardium*, was ſmall in quantity and perfectly tranſparent.

The lungs lay under the diaphragm and its muſcles in a deep cavity, formed by the five true ribs: They were pretty thick about the middle, and exceeding thin and ſharp towards the extremities.

In viewing the external eye, it ſomewhat reſembled the human, only that it was leſs convex, with a free and moveable upper eye-lid, with eye-laſhes, as moſt terreſtrial animals have; beſides a *tunica nictitans*, as in other birds. Beſides the ſeveral muſcles of the eye, as they are in brutes, it had two more, one ariſing from the fore-part of the *ſclerotica*, which ſoon formed a ſmall tendon, obliquely ſurrounding the optic nerve, and then joined to another muſcle, which ariſes oppoſite to the former, from which the tendon continues its way, and is inſerted in the *tunica nictitans*: The aqueous humour was found in greater quantity than uſual: The cryſtalline humour was of an uniform ſubſtance; but leſs convex on the inſide than on the outſide: The vitreous humour was ſmall in quantity, conſidering the largeneſs of the eye; the *choroides* was entirely black, without that variety of colours at its bottom, which is common to moſt brutes. The fore-part of the *ſclerotica*, where it is annexed to the *cornea*, was bony, conſiſting of 15 bony ſcales, joined to each other; ſo as to form one circular bone round the *cornea*.

Fig. 1. Plate XIII. (after removing the *ſternum*) repreſents the upper part of the *thorax*, with the heart and liver and neighbouring parts, in their natural ſituation; A A the membranous diaphragm, in which were obſerved ſeveral diſtinct cavities; *a a a* the ligament that ſuſpends the diaphragm; *b b* the ribs; B the heart; C C the two lobes of the liver, immediately above the heart; *c c* the brachial artery; *d* the vein; *e e* the *vena cava*; *f* a gland on the brachial artery; *g g* part of the *aſpera arteria*; *h h* part of the *oeſophagus*; *i i* two muſcles ariſing from the *ſternum*, and inſerted into the *aſpera arteria*.

Fig. 2. Repreſents the inferior part of the *thorax* (the heart and liver being removed) A A A the lower part of the diaphragm, immediately covering the lungs; B B, *&c.* 8 ſtrong fleſhy muſcles, ariſing from the ribs, and inſerted into the diaphragm, forming a cavity for the heart and liver; *c c*, *&c.* the ribs; D the deſcending trunk of the *aorta*; E E the left lobe of the lungs, freed from the diaphragm; F part of the *aſpera arteria*.

Fig. 3. Repreſents part of the globe of the eye, *a* the *cornea*; *b b* the *ligamentum ciliare*; *c c c* the fore-part of the *ſclerotica*, compoſed of 15 bones.

Fig. 4. Repreſents the back part of the globe; *a a a* the back part of the *ſclerotica*; *b b b*, *&c.* the 7 muſcles; *c c c* the 8th and 9th, whoſe tendon *d d* goes round the optic nerve *f*, and is inſerted into the *tunica nictitans*; *e e* the *membrana nictitans*.

Fig. 5. Repreſents the kidneys with their veſſels; A A the kidneys; B B the deſcending *aorta*; C C the *cava*; D D the emulgent arteries; E E the emulgent vein with its ramifications; F F the *ureters*; G the union of the ſuperior and inferior *ureter*.

The mean Motion of Mercury *and his* Nodes. *determined by a tranſit of that Planet over the Sun's Diſk*, October 29. 1723; *by Dr.* Halley. Phil. Tranſ. N° 386. p. 228.

THE tranſit of the planet *Mercury*, over the ſun's diſk, being one of the moſt curious and uncommon appearances that the heavens afford, our aſtronomers, both at home and abroad, made due preparation to obſerve, with the utmoſt exactneſs, that which happened on the 29. of *October* 1723, and which Dr. *Halley* had predicted in the year 1691 (vide *Phil. Tranſ.* N° 193) would in part be viſible in *England*; and the ſky, proving more than ordinary favourable at that time, we were enabled to obſerve the ingreſs on the ſun's limb, with the greateſt accuracy. Accord-

PLATE. XIII.

Fig. I.

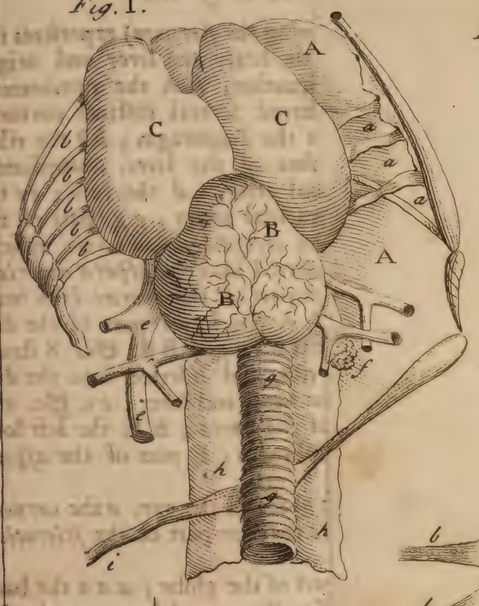


Fig. II.

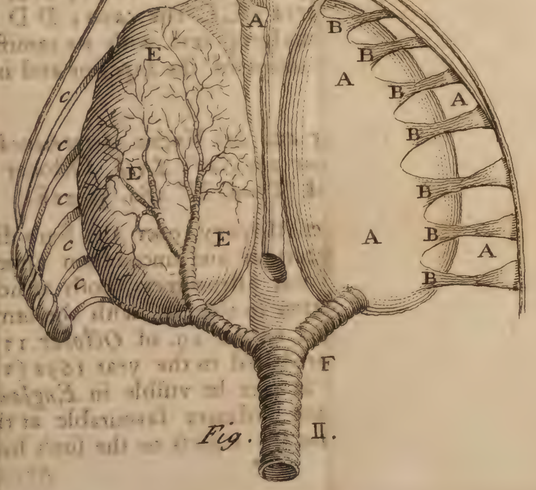


Fig. III.

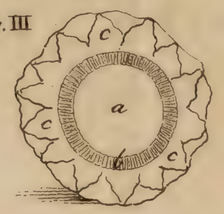


Fig. IV.

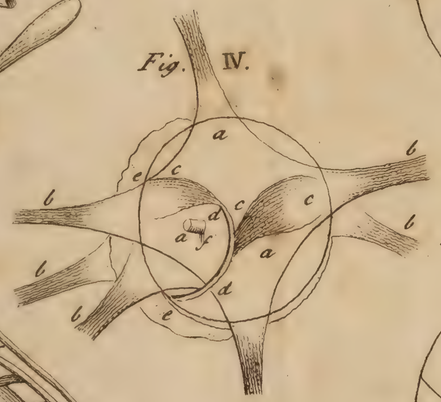


Fig. V.

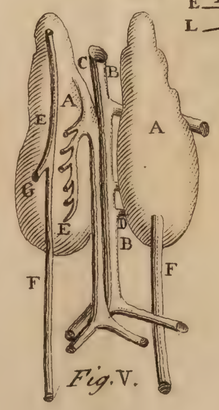


Fig. VI.

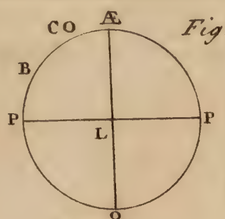


Fig. VII.

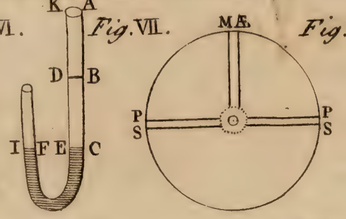


Fig. VIII.

Fig. IX.

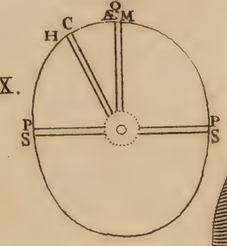


Fig. XI.

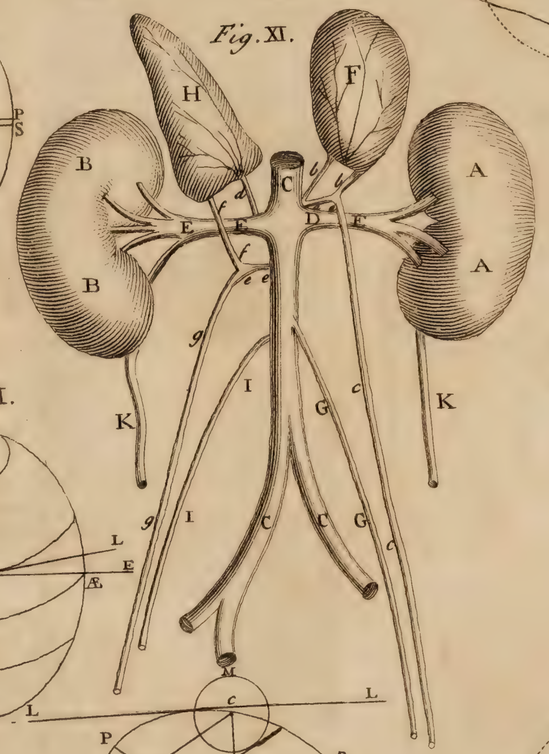


Fig. XII.

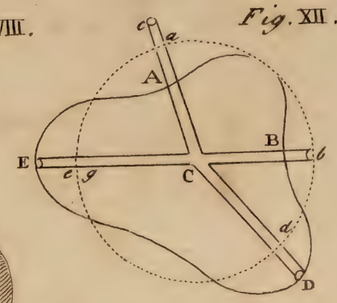


Fig. X.

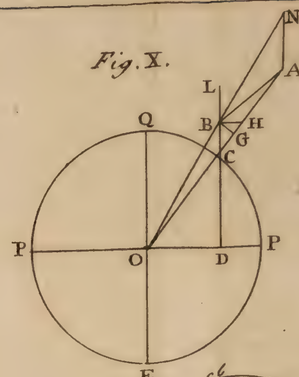


Fig. XIII.

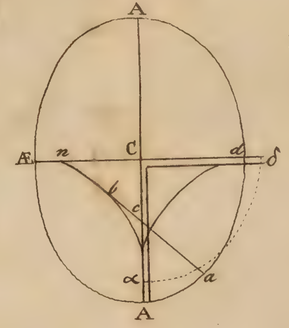


Fig. XIV.

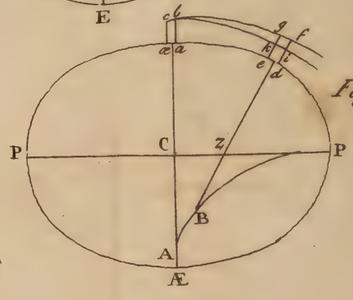


Fig. XV.

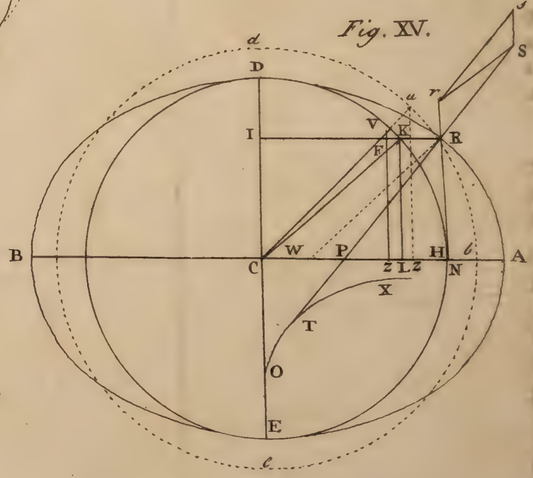


Fig. XVI.

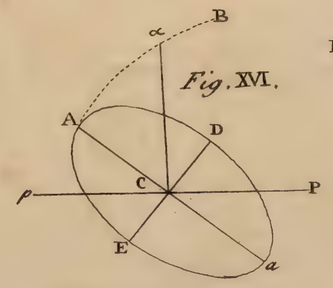


Fig. XVII.

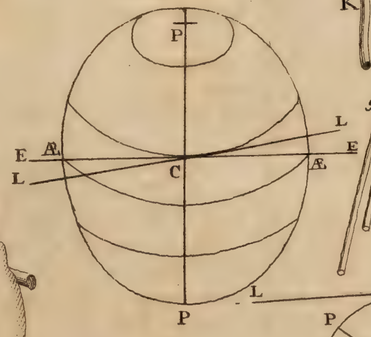
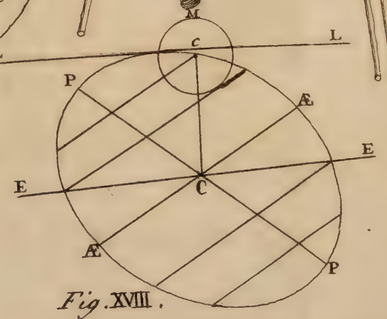


Fig. XVIII.



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Accordingly, the ſame day, *viz. October* 29. 1723. O. S. in the *Royal Obſervatory* at *Greenwich*, the Dr. firſt perceived with his 24 foot teleſcope, the planet making a ſmall notch in the ſun's limb at 2^h 41′ 23″ apparent time; and at 2^h 42′ 26″ he was entirely entered, making an interior contact, the light of the ſun's limb juſt beginning to appear behind his dark body; which, notwithſtanding the ſlowneſs of the motion, was, in a manner, inſtantaneous: Then applying the micrometer to the ſaid 24 foot tube, he opened it in ſuch a manner, as to take in 16′ 15″, equal to the ſun's ſemidiameter at that time; and cauſing the northern edge of the ſun to move exactly along one of the pointers, he waited till the center of *Mercury* came to move along the other, as he found it to do at 3^h 1′ 16″ apparent time. But refraction contracting this difference of declination about 5″ (the ſun being then but about 11° high) he concluded, that the centers of the ſun and *Mercury*, were truly in the ſame parallel of declination at 3^h 3′ apparent time nearly.

At *Wanſted* in *Eſſex*, Mr. *Bradley* obſerved with the *Huygenian* teleſcope, upwards of 120 foot long, the total immerſion or interior contact of the limbs, at 2^h 26′ 45 equat. time, that is 2^h 42′ 38″ apparent time, twelve ſeconds later than Dr. *Halley* found it at *Greenwich*; moſt of this difference was owing to the difference of meridians: And Mr. *Bradley*, applying the micrometer to that vaſt radius, meaſured the diameter of the planet 10″ 45‴. At 2^h 48′ 57″ he found the difference of declination between the ſouthern limbs of the ſun and planet by the micrometer, in a 15 foot teleſcope, to be 15′ 19″. Wherefore, allowing the obſerved ſemidiameter of the planet and the refraction, the ſaid difference was neareſt 15′ 30″; and conſequently, *Mercury* 45″ more ſoutherly than the ſun's center, in reſpect of declination.

In *Fleet-ſtreet* Mr. *George Graham* obſerved the firſt impreſſion on the ſun's limb at 2^h 41′ 9″ apparent time; and at 2^h 42′ 19″ *Mercury* was entirely within the diſk. At 3^h 6′ 41″ he meaſured with a micrometer, in a 12 foot teleſcope, the diſtance of his center from the neareſt limb of the ſun 2′ 13″. And again at 3^h 25′ 24″ their diſtance was found 3′ 57″. At 3^h 34′ 43″ he meaſured the difference of declination from the northern limb of the ſun 14′ 57″, which, corrected by refraction, becomes 15′ 4″, that is, 1′ 11″ more northerly than the ſun's center.

In the *Obſervatory* at *Paris*, S. *Maraldi* obſerved the firſt appearance of *Mercury* on the ſun's limb, at 2^h 50′ 13″ apparent

rent time, and the interior contact at $2^h\ 51'\ 48''$. And M. *de Liſle*, obſerving a-part, concluded the ſame at $2^h\ 51'\ 37''$; but ſuſpects it might have been ſome few ſeconds later: The laſt mentioned Gentleman's obſerved latitudes were, as follows.

	h.	′	″		°	′	″
At	2	56	20	the N. Lat. of *Mercury* was	3	36	
	3	0	40		3	42	
	3	10	20		3	46	
	3	16	12		3	55	

At *Bononia* in *Italy*, S. *Manfredi* obſerved *Mercury* indenting the ſun's limb at $3^h\ 26'\ 22''$, and that he was gotten entirely within it, at $3^h\ 27'\ 45''$. And theſe are the obſervations (moſt to be depended upon) Dr. *Halley* received from abroad.

In order to deduce from this phænomenon, ſo accurately obſerved, what may contribute to the perfecting of the theory of *Mercury*'s motion, which (as appears by the near agreement of the Dr's numbers with this and ſeveral other obſervations of him) ſeems to need but very little correction. The Dr. carefully computed from his tables the motion of the planet in five hours, and found his apparent motion on the ſun to be in longitude 29′ 21″ retrograde; and that his latitude increaſed northerly 4′ 17″ and ½ in the ſame time: Whence the horary motion in longitude 5′ 52″ and in latitude 51″ and ½; and thence the angle of the viſible way, with the ecliptic 8° 19′; and the horary motion in that way 5′ 56″. Again, the angle of the ecliptic with the meridian being in this place 73° 24′, the viſible way of *Mercury* made an angle of 65° 5′ with the meridian, paſſing thro' the center of the ſun, whence the horary change of declination becomes 2′ 30″ exactly.

Theſe *data* the Dr. chooſes rather to take from the theory than from immediate obſervation; becauſe there is always an unavoidable, tho' ſmall uncertainty, in what we obſerve; yet greater than there can be in the computation for ſo ſmall a ſpace of time; eſpecially now the theory is, as has been ſaid, ſo very near the truth.

This premiſed, let us now enquire into the true time of the central ingreſs, and the latitude of the planet at that time. And firſt by Dr. *Halley*'s own account, *Mercury* was gotten into the parallel of the ſun's centre 21′ and ½ after the central ingreſs, in which time he aſcended to the northward 54″; and ſo much, therefore,

therefore, was he more ſoutherly than the ſun's center at his ingreſs. Mr. *Bradley*, 7′ and ½ after the ſaid ingreſs, in which the planet aſcended 19″, found his declination 45″ ſouth; and, therefore at the ingreſs, his declination was 1′ 4″ ſouth. And by Mr. *Graham*'s obſervation, *Mercury* was more northerly than the ſun's center 1′ 11″, 53′ 20″ after the central ingreſs; but in that time, *Mercury* aſcended 2′ 13″; wherefore, according to him, at the ingreſs the planet had 1′ 2″ S. declination. We ſhall not, therefore, miſtake above a ſemidiameter of *Mercury*, if we aſſume his declination at that time to have been preciſely one minute.

Now the ſun's ſemidiameter being then 16′ 15″, 1′ is the ſine of 3° 32′ in the arch of the ſun's limb; and conſequently, the point of this ingreſs was 13° 4′ more northerly than the ecliptic: Whence the latitude of *Mercury* was then 3′ 40″ N. and difference of longitude 15′ 50″, by how much he, at that time, followed the ſun's center.

If therefore, to the arch of 13° 4′, we add the double of 8° 19′, or of the angle, which the viſible way made with the ecliptic, we ſhall have 29° 42′ for the point on the ſun's weſtern limb, at which the planet made his exit, likewiſe to the N. of the ecliptic: Hence the chord, deſcribed in the whole tranſit, was of 137° 14′, and the chord itſelf 30′ 16″; and the neareſt diſtance to the ſun's center 5′ 56″. Now the horary motion in this chord being 5′ 56″, the whole duration of this mercurial eclipſe becomes 5ʰ 6′ in reſpect to the center of the planet; and therefore, the neareſt approach of their centers was at 5h. 14′ 30″ at *Greenwich*; and the exit at 7h. 47′ and ½, both viſible in our *American* plantations, had there been any curious perſons there, qualified to obſerve them.

It likewiſe follows, by the obſerved diameter of *Mercury*, *viz.* 10″ 45‴, that he was very little leſs than 2′ of time in paſſing the limb; and by the given neareſt diſtance to the ſun's center, it is concluded, that he was in conjunction, in point of longitude at 5h. 23′ 15″, having then 6′ N. Lat. preciſely: Nor can it be doubted, but that all this would have been found exceeding near the truth, had not the too early ſetting of the ſun deprived all *Europe* of the deſirable ſight.

There being a very remarkable period of the motion of *Mercury* in 46 years, in which time, he makes 191 revolutions about the ſun; this tranſit of ours is found to have been preceeded by two others at that interval. The firſt at *Paris* when *Gaſſendus* on the 28. of *October* 1631, O. S. was the firſt that ever

ever obſerved this appearance of *Mercury*, within the ſun's diſk, and found him to paſs off at 10h. 28′ *mane*: The ſecond was *October* 28, 1677, when Dr. *Halley* himſelf had the good fortune to obſerve both the ingreſs and egreſs of the planet in the iſland of St. *Helena*; the mean time, when he was neareſt the ſun's center, being there 3′ 50″ paſt noon; and the viſible duration of the tranſit of the center of the planet 5h. 14′ 20″, which was ſome ſmall matter contracted by parallax, and moſt likely might have been 5h. 15′ without it. Now in 5h. 15′ *Mercury* deſcribed the chord of 146° 52′ in the ſun's limb, being 31′ 9″; and conſequently, the neareſt diſtance to the center was 4′ 38″, or the ſine of 16° 34′, the ſun's ſemidiameter being radius; that is, 1′ 18″ leſs than it was found in 1723. Hence alſo it follows, that the true conjunction in longitude was 7′ of time later than the neareſt approach of the centers, *viz.* at 10′ 50″ at St. *Helena* or at 35′ paſt noon at *Greenwich*; and that the N. Lat. of the planet at that time was 4′ 41″.

Suppoſing, therefore, the neareſt diſtance of the centers in the tranſit of 1631 to have been 3′ 20″, that is, 1′ 18″ leſs than in 1677, we ſhall find, that *Mercury* then deſcribed a chord of 156° 20′, traverſing the diſk of the ſun in 5h. 21′ 30″; ſo that ſuppoſing his exit at 10h. 28′ at *Paris*, that is, 10h. 18′ 40″ at *Greenwich*, he entered on the ſun at 4h. 57′ 10″ in the morning; and was neareſt his center at 7h. 38′ apparent time; but in the ſame longitude with him at 7h. 43′, or *October* 27, 19h. 43′ apparent time. having then 3′ 22″ N. Lat.

Dr. *Halley* in *Phil. Tranſ.* N° 193. for the month of *March* 1690-91; that is, upwards of 30 years before, predicted, by help of the two former, this laſt tranſit, with a ſurpriſing exactneſs, even beyond his expectation, making the time of the middle or neareſt approach of the centers of the ſun and *Mercury*, *October* 29, 1723, 5h. 19′ apparent time, which was found by obſervation at 5h. 14′ and ½, only 4′ and ½ ſooner; and in latitude, *Mercury* was but 6″ more ſoutherly than the Dr. had then computed it; the error in longitude being little more than two diameters of this exceeding ſmall planet, and in latitude but a ſingle ſemidiameter thereof. So that, for the future, aſtronomers may depend upon Dr. *Halley*'s table of theſe tranſits in *Phil. Tranſ.* N° 195. to a few minutes of time, and not wait with the uncertainty of hours, nay days, as has lately been done.

But, in order to obtain a yet farther degree of exactneſs by help of this obſervation, it may be moſt expedient to compare with

with it the ingress the Dr. observed at St. *Helena*; because in that, as well as in this, the latitudes of the planet being very small, a little error in them will not so much affect the longitudes. Supposing, therefore, that *Anno* 1677, *October* 27, 21h. 26′ 15″ at St. *Helena*, or 21h. 50′ 15″ apparent time at *Greenwich*, the center of *Mercury* entered on the sun; and that at that time, he was 8° and $\frac{1}{4}$ on the sun's limb, to the north of the ecliptic (according to what is above concluded) it follows, that he had then 2′ 20″ N. Lat. and 16′ 5″ greater longitude than the sun's center; as in the present transit, *October* 29, 2h. 41′ 30″ apparent time at *Greenwich*, he had 3′ 40″ N. Lat. and 15′ 50″ more longitude.

Now, the apparent geocentric differences of longitude are to the real heliocentric differences, as the planet's true distance from the sun, to his distance from the earth; that is, in both cases, as 313 to 676: Wherefore, in 1677, *Mercury* wanted 34′ 45″ of the conjunction with the sun; and in 1723 but 34′ 13″, at the times of his apparent ingress on the disk. And equating the times, the Dr. finds, that the sun *Anno* 1677 *October* 27, 21h. 34′ 20″ equated time was in 15° 36′ 55″ of *Scorpio*; and consequently, *Mercury*'s heliocentric place in 15° 2′ 10″ of *Taurus*: And *Anno* 1723, *October* 29, 2h. 25′ 30″ equat. time, the sun was in 16° 39′ 43″ of *Scorpio*; and therefore, *Mercury* at that time in 16° 5′ 30″ of *Taurus*.

Mercury, therefore, in 46 years, with 11 intercalations, and besides 1d. 4h. 51′ 10″, has performed 191 revolutions to the equinoctial points, and over and above 1° 3′ 20″. But by the *Scholium* to *Prop.* 14. *Lib.* 3. *Nat. Phil. Princip. Math.* the motion of the *Aphelion* of *Mercury*, from the equinox in that time, is 40′ 18″: So that there remains 23′ 2″ of true anamoly to be reduced to the mean. Now the mean anamoly of *Mercury*, in both cases, being 5 signs 12° 23′ 2″ of true anamoly, gives 15′ 24″ mean anamoly; which added to 40′ 18″ becomes 55′ 42″, for the mean motion above so many revolutions; and this is to be increased by 8″ to reduce it to the plane of *Mercury*'s orbit, in all 55′ 50″.

Hence, doubling the interval, in 92 *Julian* years 1d. 9h. 42′ 20″, the mean motion of *Mercury* from the equinox is 1° 51′ 40″, from which taking 5° 44′ 50″, the motion in 1d. 9h. 42′ 20″, we have his motion in 92 *Julian* years 11^s 26° 6′ 50″; and in 100 years 2^s 14° 2′ 13″, which is but 20″ more than the Dr. had made it some years ago, in his astronomical tables, shortly

ſhortly to be publiſhed; and differs but one hour's motion therefrom in 3000 years.

The abovementioned proportion of the diſtances, *viz.* 313 to 676 is alſo between the latitudes, ſeen from the earth and the inclinations or heliocentrie latitudes of the planet: So that 2′ 20″ at the ingreſs of 1677 gives 5′ 2″; and 3′ 40″ in 1723 becomes 7′ 55″ for the latitudes at the ſun. And the inclination of the orbit of *Mercury* to the plane of the ecliptic (determined by accurate obſervations near his northern limit) being 6° 59′ 20″, we compute the diſtance of the planet from his node, in the former 41′ 7″, and in the latter 1° 4′ 37″; which deducted from his heliocentric places reſpectively, leave the place of the aſcending node in 1677 14° 21′ 3″ of *Taurus*; and in 1723 15° 0′ 53″ of *Taurus*: So that in 46 years the node is found 39′ 50″ forwarder in the ecliptic; which is but 1′ 30″ more than the preceſſion of the equinox in the ſame time; we may, therefore, ſafely aſſume the plane of *Mercury*'s orbit to be immoveable in the ſphere of fixed ſtars, and its aſcending node to be 15° 41′ from the firſt ſtar of *Aries*: Nor can ſo very ſlow a motion (ſuppoſing any ſuch) be fully defined; but by the utmoſt care and diligence of future aſtronomers, after the obſervation of many ages.

As to the reſt of the theory of this planet's motion, Dr. *Halley* makes his mean diſtance from the ſun, 38710 ſuch parts, as the mean diſtance of the ſun and earth is 100000; and his greateſt equation 23° 42′ 37″. The *epocha* of his mean motion in the beginning of the year 1723, O. S. from the equinoctial point, he makes 19° 9′ 31″ of *Sagittarius*; and that of his *aphelion* to the ſame time 13° 3′ 34″ of *Sagittarius*; the *aphelion* moving, according to the order of the ſigns, 7 minutes in 8 years. And theſe numbers, the Dr. preſumes, may repreſent the motion of *Mercury*, with an exactneſs equal to that of any of the other planets; perhaps, as near as the ſun's place by any tables, or thoſe of the fixed ſtars by any catalogue yet extant.

It were to be wiſhed, that ſome good obſervation, like this, had been made of the like tranſit of *Mercury* at his other node in *April*; where he was ſeen it is true, *April* 23, 1661, but ſo imperfectly, that neither ingreſs or egreſs was any where obſerved; and tho' it be certain, that he traverſed the ſun on *April* 26, 1674; and again *April* 24, 1707; yet we were ſo unfortunate, that the conjunction in both happened ſo near mid-

night,

night, that he escaped unobserved by all the astronomers in *Europe*, excepting only M. *Roemer* at *Copenhagen*, whose observation is, as follows. 'This day, *viz.* *May* 6, 1737 at 4h. '19′ in the morning, *Mercury* was observed just going off the 'sun's limb; being $\frac{1}{4}$ of the sun's diameter above the lowest 'limb, and to the left in an inverting telescope; by reason of 'the too short *mora* we could not determine these things with 'greater accuracy.' It was great pity, that he did not at least estimate, how many diameters of his body he was distant from the sun's limb, or what part of the diameter, if so near. But Dr. *Halley*, having examined this observation, finds, that the sun at that time, was but just risen, or rather rising, and soon after entered into a cloud; so that the sun's limb could not be distinctly seen, it always undulating and sparkling much, when so near the horizon; in which circumstance, a just observation could hardly be made.

Let us now see, how the Dr's numbers, corrected as above, will represent this observation. *Anno* 1707. *April* 24. 16h. 19′ at *Copenhagen* is 15h. 28′ at *Greenwich*; but 15h. 24. 20″ equated time. To this time the Dr. finds the sun's true place 14° 50′ 1″ of *Taurus*, and his distance from the earth 101005. The correct *epocha* of *Mercury*'s mean motion for the year 1707 is 3^s 13° 18′ 45″, to which adding for the rest of the time 3^s 19° 9′ 28″, we shall have his mean motion at the time of the observation 2° 28′ 13″ of *Scorpio*; and taking his *aphelion* in 12° 49′ 49″ of *Sagittarius* therefrom, we have his mean anamoly 10^s 19° 38′ 24″, and thereby the equation to be added 12° 39′ 41″; and thence the place of *Mercury* in his orbit 15° 7′ 54″ of *Scorpio*: But the correct place of the descending node is 14° 46′ 25″ of *Scorpio*; and therefore, *Mercury* being 21′ 29″ past the node, had 2′ 36″ S. Lat. at the sun; and his place reduced to the ecliptic was 15° 7′ 45″ of *Scorpio*, that is, 17′ 44″ past the conjunction of the sun, which diminished in the proportion of 5567 to 4533, or of the distance of the planet from the earth to his distance from the sun, becomes 14′ 27″; and by so much was he past the conjunction, as viewed from the earth. Again, by the same proportion, his geocentric latitude at that time was 2′ 7″ S. and therefore, his apparent distance from the sun's center, was 14′ 37″, that is, but 1′ 18″ from his western limb: So that he might well be said, *jamjam exiturus*.

But that *Mercury* should at that time be so far northerly, as M. *Roemer*'s words import, was absolutely impossible; and Dr. *Halley* is apt to think, that so acute an astronomer, as

M. *Roemer* was, could not himſelf be the obſerver; but ſome perſon leſs acquainted with theſe matters; which the words *ſpectabatur Mercurius*, inſtead of *Mercurium vidi*, ſeem to import. If he then had had N. Lat. he muſt needs have been ſeen in the ſun in *April* 1720, which we are aſſured he was not.

Laſtly, the Dr. advertiſes, that on the laſt day of *October* 1736, *Mercury* will again traverſe the northern part of the ſun's diſk, both ingreſs and egreſs being viſible to all *Europe*.

A Continuation of the Account of the Figure of the Earth; *by Dr.* Deſaguliers. Phil. Tranſ. N° 387. p. 259.

HOW the figure of the earth is deduced from the laws of gravity and centrifugal force, is very well ſhewn by Dr. *John Keill* in a book he publiſhed in 1698, againſt Dr. *Burnet*'s theory of the earth: It is true, he has made a miſtake in that book, as to the meaſure of the degrees of an ellipſis; but all relating to the oblate ſpheroidical figure of the earth is right; and the ſmall difference of taking 15 *Paris* feet for the ſpace a body falls thro' in a ſecond, inſtead of 15 feet 1 inch and 2 lines; and a number of feet, a little leſs than true, for the diameter of the earth (which was not ſo well known at that time) will noways invalidate his demonſtration: His words are, as follows

'To prove the earth higher at the equator than at the poles, 'I will firſt ſuppoſe, that at the beginning of the world the 'earth was fluid and ſpherical; but afterwards God Almighty, 'having given it a motion round its own axis, all bodies upon 'the earth would deſcribe either the equator, or circles parallel 'to the equator; and conſequently, they would all endeavour 'to recede from the center of their motion.

'It is here to be obſerved, that if a body doth freely revolve 'in a circle about a center, as the planets do about the ſun, that 'its centripetal force (or that force by which it is drawn towards 'the center) is always equal to the force, by which it endea-'vours to recede from the center: For, the force which detains 'a body in its orbit muſt be equal to the force by which it en-'deavours to recede from its orbit and fly off in the tangent. 'This may be made clear by a body turned round a center by 'the help of a thread, which retains the body in its orbit; the 'thread being ſtretched by the motion of the body, will endea-

'vour

'vour to contract itſelf equally towards both ends, by which it 'will pull the center as much towards the body, as it does the 'body towards the center.

'Now this centrifugal force is always proportional to the pe-'riphery, which each body deſcribes in its diurnal motion by '*Huygens*'s firſt *Theor. de vi centrifugâ*: So that under the 'equator, which is the greateſt circle, the centrifugal force 'would be greateſt, and ſtill become leſs, as we approach the 'pole, where it quite vaniſhes; there being no diurnal rotation 'there. And without doubt, all bodies having this centrifugal 'force, by which they endeavour to recede from the center of 'their motion, would fly off from the earth, if they were not 'kept in their orbit by their gravity, or that force by which 'they are preſſed towards the center of the earth, which is 'much ſtronger upon our earth than the centrifugal force; and 'becauſe the gravity upon the ſurface of the earth is always the 'ſame; but the centrifugal force alters and becomes leſs, the 'nearer we come to the poles; it is plain, that the gravity under 'the equator, having a greater force to oppoſe it, than that 'which is near the poles, will not act ſo ſtrongly in the one 'place, as in the other; and conſequently, bodies will not be 'ſo heavy under the equator, as at the poles. If the circle 'ÆPQP (Fig. 6. Plate XIII.) repreſent the earth, ÆQ the 'equator, and PP the poles; if C be a body in the equator; 'it is evident, that it will be pulled by two contrary forces; 'namely, that of its gravity, which pulls it towards the cen-'ter, and that of its centrifugal force, which pulls it from it. 'Now, if both theſe forces were equal, it is evident, it would 'go neither of theſe ways: But if one were ſtronger than the 'other, it would move where the ſtongeſt force pulls it; but 'only with a velocity, which is proportional to the difference 'of theſe two forces; and therefore it would not deſcend ſo 'faſt, as if there were no centrifugal force, pulling againſt it; 'that is, a body in the equator preſſes leſs towards the center 'than at the pole; where there is no centrifugal force to leſſen 'its gravity: Bodies, therefore, of the ſame denſity, are not ſo 'heavy in one place as in the other.

'Now in a ſpherical fluid, all whoſe parts gravitate towards 'the center, I think it is evident from the principles of hydro-'ſtatics and fluidity, that all thoſe bodies, which are equally 'diſtant from the center, muſt be equally preſſed with the 'weight of the incumbent fluid; and if one part come to be 'more preſſed than another; that which is moſt preſſed will

'thruſt

'thruſt out of its place that which is leaſt, till all the parts 'come to an *æquilibrium* one with another; and this is known 'by a common and eaſy experiment: If you take a recurved 'tube, (repreſented Fig. 7.) and fill it with water or any 'other fluid, it will riſe equally in both legs of the tube; ſo 'that the ſurfaces C E and F I are equally preſſed by the in-'cumbent columns B C E D and G F I H; but if one of the 'legs of this tube ſhould be filled with oil, or ſome other 'lighter fluid, and the other with water; the lighter fluid 'will riſe higher than the other: For, otherwiſe theſe ſur-'faces, which are equally diſtant from the center, would not 'be equally preſſed.

'Juſt ſo, if P Æ M P S (Fig. 8.) repreſent a fluid ſphere, 'which we may imagine compoſed of a great many commu-'nicating canals or tubes, the fluid of every one of which 'preſſes upon the center: Now, if the fluid, in every one 'of theſe tubes, was of equal weight or gravity, it is plain, 'that by that means they would likewiſe be of an equal 'height from the center: For, by that means only, would the 'center be equally preſſed by the weight of all the tubes; 'but if the fluid in the canal Æ O M were lighter than the 'fluid in the canal P O S, it is plain, that in this caſe, the 'fluid P O S preſſing more upon the center, than the fluid in 'the canal Æ O M, the ſurface of the fluid Æ O M will riſe 'to a greater height or diſtance from the center: So that by 'its greater height, which compenſates its leſſer gravitation, 'it will preſs equally upon the center with the fluid in the 'canal P O S.

'After the ſame manner, if the fluid in the canal G O H '(Fig. 9.) were heavier than the fluid in the canal Æ O M; 'but lighter than that in P O S, then would the canal G O H 'be ſhorter than Æ O M, but longer than P O S, and the fi-'gure, compos'd of all theſe tubes, would be in the form of a 'ſpheroid, generated by the circumrotation of a ſemi-ellipſis 'round its axis: But as has been already ſhewn, if Æ O M 'repreſent the ſemidiameter of the equator, all bodies in it 'are lighter than in P O S, the axis of the equator, taking 'the diameter and axis here, not as pure mathematical lines, 'but as ſmall canals or tubes; and juſt ſo thoſe bodies which 'are in the tube G O H, I have prov'd to be lighter than 'thoſe in P O S, but heavier than the bodies in Æ O M; the 'centrifugal force in G H being leſs than that in Æ M, and

'there

'there is no centrifugal force in the poles P S. It is 'plain, therefore, that the tube Æ O M will be longer than 'G O H, and G O H longer than P O S, that is, the diame-'ter of the equator will be longer than the axis of the earth; 'and consequently, the figure of the earth will be in the form 'of a broad ſpheroid, generated by the rotation of a ſemi-'ellipſis round its leſſer axis. This, I hope, will be ſufficient 'to convince the Theoriſt of the falſeneſs of his own aſſer-'tion; ſince it is plain demonſtration, that an earth, form'd 'from a chaos, muſt have a very different figure from what 'he ſuppoſes it had.

'But I will now proceed farther, and enquire how much 'the gravity is diminiſh'd at the equator, or any other paral-'lel by the centrifugal force, which all bodies acquire by 'being turn'd round the earth's axis, that from thence we 'may endeavour ro determine, what proportion the diameter 'of the earth's equator hath to its axis, to calculate which, 'I will firſt ſuppoſe, that the mean ſemi-diameter of the 'earth is 196,115,800 *Paris* feet, according to the late ob-'ſervations of the *French* Mathematicians; and ſince the 'earth turns round its axis in the ſpace of 23 hours 56'; for, 'in that time the ſame meridian returns to the ſame immove-'able point of the heavens again (but the ſun, in the mean 'time, ſeeming to be mov'd a degree, according to the order 'of the ſigns, is the cauſe, why there are four minutes more 'requir'd, before the meridian can overtake him) thence it 'follows, that a body under the equator moves thro' 1426,88 'feet, in the ſpace of one ſecond of time.

'Now, according to the theorem given us by Sir *Iſaac* 'Newton in his *Philoſ. Natural. Mathem. Schol. Prop.* 4. '*lib.* 1. the centrifugal force of any body, has the ſame pro-'portion to the force of gravity, that the ſquare of the arch, 'which a body deſcribes in a given time, divided by its dia-'meter, has to the ſpace, thro' which a heavy body moves, 'in falling from a place in which it was at reſt in the ſame 'time; and ſuppoſing a heavy body falls 15 foot in a ſecond 'of time, by calculation, it will thence follow, that the force 'of gravity has the ſame proportion to the centrifugal force 'at the equator, that 289 has to unity; and therefore, by this 'centrifugal force, which ariſes from the diurnal rotation of 'the earth round its axis, any body, placed in the equator, 'loſes $\frac{1}{289}$ part of its gravity, which it would have, were the 'earth at reſt; or which is the ſame thing, a heavy body,

'placed

'placed at either of the poles (where there is no diurnall 'rotation, and consequently, no centrifugal force) which 'weighs 289 pounds, if it were brought to the equator, would 'weigh only 288 pounds.

'Having thus determined the proportion of the centrifugall 'force at the equator, to the force of gravity, it will be easy 'from thence to shew their proportions in any parallel; for, 'it is compounded of the proportion of 1 to 289, and of the 'co-sine of Lat. to the radius; for, if two bodies describe 'different peripheries in the same time, their centrifugall 'forces are proportional to their peripheries, or to the semi-'diameters of these peripheries, as is determined by M. 'Huygens in his *Theoremata de vi centrifugâ & motu circulari*; but the periphery, which a body in the equator 'describes, has its semi-diameter equal to the radius or semi-'diameter of the earth, and in any other place, the parallels 'in which bodies move, have the co-sines of their latitude 'for the semi-diameter; and therefore it will follow, that 'the force of gravity is to the centrifugal force in a pro-'portion, compounded of the radius to the co-sine of the 'radius to the co-sine of the latitude, and of 289 to 1; and 'therefore, at the Lat. of 51° 46′ (for instance) it will be as '$\frac{466}{1}$ to 1.

'But we must observe, that it does not from thence follow, 'that a body in that Lat. loses $\frac{1}{466}$ part of its absolute gravity, 'which it would have, were the earth at rest. For, that 'could not be, unless the centrifugal force acted directly con-'trary to the force of gravity, which it doth no where, but 'at the equator; for, let the circle QPE (Fig. 10.) represent 'the earth, QE the diameter of the equator, O its center; 'and let B represent a body, which we suppose to hang by 'the thread AB, and to be placed any where between the 'pole P and the equator Q; and let BD be drawn perpendi-'cular to the axis. It it plain, that if the earth had had no 'diurnal rotation, the body B would draw the thread AB 'into the position of AC; since by that means it descends as 'near as it can to the center; and there it would stretch the 'thread with all the force of its gravity; or if we will suppose 'that the centrifugal force acted, according to the same direc-'tion AC, it would then directly oppose the force of gravity, 'and the thread would remain in the same position, but it 'would be stretched with a force proportional to the diffe-'rences of these two forces: But because the body B turns

'round

'round the center D, it will endeavour to recede from it 'according to the line CB, in which direction the centrifugal 'force acting, it will not directly oppose the force of gravity; 'but it will draw the thread from the position AC into the 'position AB; let BG be drawn perpendicular to AC; if 'BC represent the centrifugal force, acting according to the 'direction BC, it is equivalent (as is commonly known) to 'two forces, one of which is as GC, and acts according to 'the direction CG, which is contrary to that by which it 'descends to O; the other is as GB, and acts according to 'the direction GB, which is no ways contrary to the force of 'gravity. If therefore, BC represent the total centrifugal 'force of the body B, that part of it, which directly opposes 'the force of gravity, will be GC: Whence it follows, that 'the decrease of gravity in going from the pole to the equa- 'tor, is always as the square of the co-sine of the latitude; 'for, draw BH, parallel to the axis PP; and because the 'triangles HCB, CDO are equiangular; therefore HC is 'to CB, as CO is to CD, or as QO is to CD; but QO is 'to CD, as the decrease of gravity at Q is to the centrifugal 'force at C; and therefore, HC is to CB as the decrease of 'gravity at Q is to the centrifugal force at C: But if CB 'represent the centrifugal force at C, GC will represent that 'part of it, which acts directly against the force of gravity; 'and consequently, the decrease of gravity at the equator is 'to the decrease of gravity at C, as HC is to GC: Now, 'HC is to GC in a duplicate ratio of HC to CB, or of CO or 'OQ to CD by *Eucl.* 8. 6. and therefore, the decrease of 'gravity at Q is to the decrease of gravity at C, as the square 'of CO is to the square of CD, Q. E. D.

'Whence it is plain, that if HC represent the decrease of 'gravity at the equator, and GC its decrease at C, then will 'GH represent the difference of these two diminutions, or 'the difference between the gravity at Q and the gravity at 'C; but HC is to HG in a duplicate ratio of HC to HB, or 'of OC to DO; that is, the decrease of gravity at the 'equator is to its increase at C, as the square of the radius is 'to the square of the sine of the latitude.

'By this it will likewise appear, that the direction of heavy 'bodies is not to the center of the earth, as has been always 'suppos'd; for, if we take a heavy body and hang it by a 'thread, the thread produced will not pass thro' the center 'any where, but at the poles and the equator; for, in the

'figure, the thread is carried by the centrifugal force of the 'body B, from the poſition A C into the poſition A B, where 'it will reſt.

'Now, to determine the angle C A B, which the line of 'direction of the body makes with the line A C, let A N be 'drawn parallel to B C, and produce O B, till it meet with it 'in N, and let us conſider the body B, as drawn by three 'powers, according to three different directions B O, B L, 'and A B; the power, whtch pulls it, according to B O, is 'its gravity; that which draws it, according to the direction 'B L, is its centrifugal force; and that which acts according 'to A B is the ſtrength of the thread, by which the body is 'hinder'd to move, according to either of the other two 'powers: But by a *Theorem*, which is demonſtrated by 'ſeveral of the writers of mechanics; but particularly, by 'M. *Huygens* in his ſmall treatiſe *de potentiis per fila tra-* 'hentibus; if a body be pull'd by three different powers, 'which are in *æquilibrio* with one another, according to three 'different directions A B, B L, and B O; theſe three powers 'will be, as the three ſides of the triangle A B N, *viz.* as 'A B, A N, and B N reſpectively, or as A B, B C and A C; 'B N being very near parallel, and conſequently, equal to 'A C, ſince they do not meet, but at a great diſtance. Hence 'it follows, that the force of gravity is to the centrifugal 'force, as A C to B C. But a method has been already 'ſhewn, how the proportion of the force of gravity to the cen- 'trifugal force may be determined; and therefore, the pro- 'portion of A C to B C may be alſo determined, which at 'the Lat. of 51° 46′ is as 466 to 1. Therefore, in the tri- 'angle A B C, the proportion of A C to B C is known, and 'the angle A C B being equal to the angle C O Q, which is 'ſubtended by the arch C Q, the latitude of the place; from 'thence by the tables of ſines and tangents, the angle B A C 'may be known, which in the above-mentioned Lat. is about 'five minutes.

'Hence alſo it will appear, that it is not the line A C, 'which being produced, paſſes thro' the center; but the line 'A B that is perpendicular to the curve P Q: For, all the 'particles of the fluid will ſettle themſelves in ſuch a poſition, 'that all their lines of direction downwards, muſt be per- 'pendicular to the ſurface of the body which they compoſe; 'for, otherwiſe the parts of the fluid would not be in equi- 'librio with one another, and therefore, tho' the lines of direc-

tion

'tion of heavy bodies do not pass thro' the center of the earth; 'yetare they still perpendicular to their horizons; and upon 'this account there could arise no error in levelling of lines, 'and in finding the risings and fallings of the ground.

'Upon this account also it will appear, that the surface of 'of the earth is not spherical: For, if it were, then would 'all lines, drawn from the center, be perpendicular to the 'surface of the earth; since it is the known property of a 'sphere, that they must be so: But I have already shewn, 'that it is not so in the earth; and therefore, it is plain, that 'the earth is not a sphere. That, therefore, I may enquire 'more particularly into the figure of the earth I will resume 'my former hypothesis, *viz.* that the earth is compos'd of an 'infinnite number of canals, which communicate with one 'another at the center, and are equiponderant; of which we 'will consider two, as OQ and OC; and let OQ be $= r$, 'OD $= x$ and DC $= y$; let the absolute gravity be call'd p, 'and the centrifugal force at the equator n. OC is '$= \sqrt{x^2 \times y^2}$ the weight of the canal; OQ is equal to the abso-'lute gravity of the whole canal minus the centrifugal force 'of each particle in it; and because the centrifugal force of each 'particle is, as its distance from the center; and therefore, it 'increases in an arithmetical progression, the greatest of which 'is n; consequently, the sum of all the centrifugal force is 'equal to the $\frac{1}{2} nr$: But upon the hypothesis, that gravity is 'the same at all distances from the center, the absolute 'gravity of the canal OQ is pr; and therefore, its real 'weight upon the center OQ is $pr - \frac{1}{2} nr$: After the same 'manner the absolute gravity of the canal OC is $p \times \sqrt{x^2 + y^2}$; 'but the sum of all the centrifugal forces of all the fluids in 'the canal OC is equal to the centrifugal force of the fluid 'in CD (as may be easily prov'd from the consideration of 'inclin'd planes) but the centrifugal force at C being to the 'centrifugal force at Q, as CD is to OQ (that is as y is to r) 'the centrifugal force at C will be equal to $\frac{ny}{r}$; and be-'cause the centrifugal force of each particle is, as its distance 'from the point D, which is the center of the circle that the 'fluid in the canal CD describes; and therefore, the centri-'fugal forces, in reckoning from the point D, must increase 'in an arithmetical progression, the greatest of which is $\frac{ny}{r}$;

and therefore, the sum of all the centrifugal forces in C D, 'must be equal to $\frac{nyy}{2r}$; therefore, the weight of the 'canal O C is $= p\sqrt{x^2 + y^2} - \frac{1}{2}\frac{nyy}{r} = pr - \frac{1}{2}nr$; which 'equation expresses the nature of the curve, made by the 'section of the earth with a plane thro' its poles; and by this 'the proportion of the axis of the earth to the diameter of 'the equator may be easily determined: For, when C O coin-'cides with O P, then C D or y becomes equal to nothing; 'and the equation is $p\sqrt{x^2} = pr - \frac{1}{2}nr$, or $px = pr - \frac{1}{2}$ 'nr; and therefore, by *Eucl.* 16. 6. p has the same propor-'tion to $p - \frac{1}{2}r$, that r has to x, or O Q to O D; but p is to '$p - \frac{1}{2}n$, as 289 is to 288 and $\frac{1}{2}$, or as 578 is to 577; which 'therefore, is the proportion of the greatest diameter of the 'earth, to the least: But this is on supposition, that gravity 'is the same at all distances from the center; but if we will 'suppose, that the gravity of bodies without the earth, is in 'a reciprocal ratio to the squares of their distances from the 'center; the gravity of those bodies, which are within the 'earth, will be directly as their distance; both which do best 'agree with the observ'd phænomena of nature; then will 'the gravity at the equator be to the gravity at the poles; as '689 to 692; which numbers, in this hypothesis, do also 'express the proportion of the diameter of the earth, drawn 'thro' its poles, to its diameter drawn in the plane of the 'equator.

'It is upon account of this diminution of gravity, according 'as we approach the equator, that pendulum's of the same 'length in different latitudes, take different times to perform 'their vibrations; for, because the accelerating force of 'gravity is less at the equator than under any parallel; and 'under any parallel it is still less than under another, which 'is nearer the poles; thence it plainly follows, that a body 'placed in the equator, or in any other parallel, will take a 'longer time to descend thro' an arch of a given circle, than 'it would do at the poles; and the farther a body is removed 'from the poles, the longer time it will take to descend thro' 'any given space.

'Hence it follows, that the length of pendulums, which 'perform their vibrations in equal times in different lati-'tudes, are directly, as the accelerating forces of their gravi-'ties: For, the time, a body takes to descend thro' the

'arch

'arch of a cycloid, is to the time it will take to fall thro' the 'axis of the cycloid, always in a given proportion, *viz.* as the 'semi-periphery of a circle is to its diameter, by the 25th 'prop. of *Huygens*'s *Horologium oscillatorium*; and therefore, 'when the times in which a body descends thro' the axes of 'two different cycloids are equal, the times of the descent thro 'the cycloids will be also equal; but when the times of the 'descent thro' the axes are unequal, these axes, and conse-'quently, the lengths of the pendulum, which vibrates in 'these cycloids, are proportional to the accelerating forces of 'their gravities.

'By this if we know the length of a pendulum, which per-'forms its vibrations in a given time, in any one part of the 'earth, it is easy to determine the length of a pendulum, 'which performs its vibrations in the same time, in any 'other part of the earth: As for example, the length of a 'pendulum, which vibrates seconds at *Paris*, is three foot 'eight lines and half; let it be requir'd to find the length of 'a pendulum, which vibrates seconds at the equator; because 'the graviry at the poles is to the gravity at the equator, as '692 is to 686; therefore, the decrease of gravity at the 'equator is $6\frac{3}{92}$ parts of the whole gravity: But, as I have 'before demonstrated, the decrease of gravity at the equator 'is to its increase in any other Lat. as the square of the radius 'is to the square of the sine of the Lat. Now, the lati-'tude of *Paris* being 48° 45′, its sine is 75, 183; and 'therefore, the square of the radius is to the square of 'the sine of the latitude. as 1000000 to 565248: But as '1000000 is to 565248, so is 3,000, the number which 'represents the decrease of gravity at the equator, to '1,695, the number which represents its increase at *Paris*; 'which added to 689, the gravity at the equator, makes '690,695, the number which will represent the gravity 'at *Paris*. But I have already shewn, that as the gra-'vity at *Paris* is to the gravity at the equator, so is the length 'of a pendulum, which vibrates seconds at *Paris*, to the 'length of a pendulum that vibrates seconds at the equator, 'that is, as 690,695 to 689, so is 36,708, the length of a pen-'dulum at *Paris*, which performs its vibration in a second, to '36,616; which, therefore, is the length of a pendulum, which 'performs its vibrations in a second at the equator: So that the 'difference between these two pendulums is $10\frac{22}{50}$ parts of an 'inch, which comes pretty near the observations of M. *Richer*, 'who, at the island of *Cayenne*, whose Lat. is 5°, found,

that

'that a pendulum which vibrates ſeconds there, was a tenth 'part of an inch ſhorter than a pendulum, which vibrates ſe- 'conds at *Paris*.

'Thus we ſee, that the principles and hypotheſis, and withal 'their conſequences, upon which the broad ſpheroidical figure 'of the earth is founded, do exactly agree with obſervations; 'and therefore, there is no doubt to be made; but that the 'earth is really of ſuch a figure; and that the hypotheſis, upon 'which this figure is founded, (*viz.* the diurnal rotation of the 'earth, and conſequently, the centrifugal force of all bodies 'upon it) muſt be admitted for a true one: Since the different 'vibrations of pendulum's of the ſame length in different lati- 'tudes, can depend upon no other cauſe; for, the change of air 'cannot produce any ſuch effect. For, if the air really cauſed 'any alterations in the vibrations of a pendulum, it would pro- 'duce a quite contrary effect to what is obſerved; for, pendu- 'lum's near the equator would move faſter than they would do 'in places of greater Lat. the air in the one place, being more 'rarified is much thinner and finer than it is in the other; and 'therefore, gives leſs reſiſtance to bodies that move in it.

'In this reaſoning, we have ſuppoſed the earth to have been 'at firſt fluid, as the *Theoriſt* has done before us; but if we 'will put the caſe, that the earth was firſt partly fluid and 'partly dry, as it is at preſent; yet becauſe we find, that the 'land is very near of the ſame figure with the ſea (only raiſed 'a little higher, that it might not be overflowed) compoſing 'with it the ſame ſolid; and I have already ſhewn, that the 'ſurface of the ocean is ſpheroidical and not ſpherical, there is 'no doubt to be made, but that the land was formed into the 'ſame figure, by its wiſe Creator at the beginning of the world: 'For, if it were otherwiſe, then would the land towards the 'equator have been overflowed with water; which, as I have 'alread proved, muſt have been higher at the equator than at 'the poles; and therefore, the ſea would riſe there, and ſpread 'itſelf like an inundation upon all the land.'

Dr. *Deſaguliers* comes next to compare the experiments and obſervations, made uſe of to confirm each of the abovemen- tioned opinions.

To prove M. *Caſſini*'s figure of the earth; we muſt take the altitude of a ſtar nearer than to two ſeconds; becauſe two ſe- conds anſwer to 32 toiſes on the ſurface of the earth, and the difference of the length of degrees is but 31: And what is more, we muſt take this angle with an inſtrument of 39 inches

radius

radius; becauſe the 10 foot ſector was only uſed at the ends of the two parts of the meridian.

To diſprove M. *Caſſini*'s hypotheſis; we need only obſerve, whether a plumb-line forms an angle of five minutes with a perpendicular to the ſurface of ſtagnant waters, or lines of level.

To prove M. *Caſſini*'s hypotheſis, the height of a great many mountains muſt be accurately meaſured by trigonometry, which mathematicians have always found very difficult.

To prove Sir *Iſaac Newton*'s hypotheſis; we are only to meaſure about one tenth of an inch in a rod of 39,129 inches; and to know what to allow for the lengthening of the ſame rod by the ſummer heat, when it is ſhut up in a caſe, and carried towards the equator. For, tho' the experiments on pendulum's made by ſeveral perſons that travelled ſouthwards, differ among themſelves, yet they all agree in this, that the obſervers were obliged to ſhorten their pendulum's, in order to make them ſwing ſeconds, as they went towards the equator. And when we come to compare them together, in order to have the exact proportion of length in different latitudes, we muſt rely on the moſt exact experimenter, which we may very well do on M. *Richer*; becauſe when he found a difference, he was ſo careful to find out how much it was, that he cauſed a ſimple pendulum to ſwing, and compared it with a good pendulum-clock, which he did ſeveral times every week for 10 months together; and when he returned to *France*, he compared it with the length of the pendulum at *Paris*, which is of 3 feet 8 lines and $\frac{3}{5}$ (or 39,129 *Engliſh* inches) and found it to be ſhorter by 1 line and $\frac{1}{4}$.

An Eſſay on the Natural Hiſtory *of* Whales, *with a particular Account of the* Ambergris, *found in the* Sperma ceti Whale; *by Mr.* Paul Dudley. Phil. Tranſ. N° 387. p. 256.

THE following account relates only to ſuch whales, as are found on the coaſt of *New England*; and of theſe there are divers ſorts.

The right, or whale-bone whale is a large fiſh, meaſuring 60 or 70 feet in length, and very bulky, having no ſcales, but a ſoft fine ſmooth ſkin, no fins; but only one on each ſide, from 5 to 8 feet long, which they are not obſerved to uſe; but only in turning themſelves, unleſs while young, and carried by the dam on the flukes of their tails; when with thoſe fins they claſp about

bout her ſmall, and ſo hold themſelves on. This fiſh, when firſt brought forth, is about 20 feet long, and of little value, but then the dam is very fat. At a year old, when they are called *ſhort-heads*, they are very fat, and yield to 50 barrels of oil; but by that time the dam is very poor, and called a *dry-skin*, and will not yield more than 30 barrels of oil, tho' of a large ſize. At two years old, they are called *ſtunts*, being ſtunted after weaning, and will then generally yield from 24 to 28 barrels. After this, they are called *ſcul fiſh*, their age not being known; but only gueſſed at by the length of the bone in their mouths. The whale-bone, ſo called, grows in the upper jaw on each ſide; and is ſometimes 6 or 7 feet in length. A good large whale has yielded 1000 weight of bone. It is ſuppoſed by ſome, that the hairy part of the whale-bone and which is next the tongue ſerves for a ſtrainer of the food.

A whale's eye is about the bigneſs of an ox's, and ſituated in the hinder part of the head on each ſide, and where the whale is broadeſt; for, his head tapers away forwards from his eyes, and his body tapers away backwards; his eyes are more than half way his depth, or neareſt his under-part; juſt under his eyes are his two fins abovementioned, he carries his tail horizontally, and with that he ſculls himſelf along.

The intrails of this whale are made and ſituated much like thoſe of an ox, and their ſcalps are ſometimes found covered with thouſands of ſea-lice. One of theſe whales has yielded 130 barrels of oil, and near 20 out of the tongue. The whale-bone whale is the moſt valuable, excepting the *ſperma ceti* whale.

The ſcrag-whale is near a kin to the fin-back; but inſtead of a fin upon his back, the ridge of the hinder part of his back is ſcragged with half a dozen knobs; he comes neareſt the right whale in figure and for quantity of oil; his bone is white, but will not ſplit.

The fin-back whale is diſtinguiſhed from the right whale, by having a large fin on his back from two foot and ½ to four foot in length, whence he has his name; he has alſo two ſide-fins, as the whale-bone whale; but much longer, meaſuring 6 or 7 feet. This fiſh is ſomewhat longer than the other; but not ſo bulky; much ſwifter, and very furious, when ſtruck, and held with very great difficulty; their oil is not near ſo much, as that of the right whale, and the bone of little value, being ſhort and knobby: The belly of this whale is white.

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The bunch or hump-back whale is diſtinguiſhed from the right whale, by having a bunch ſtanding in the place, where the fin does in the fin-back. This bunch is as big as a man's head, and a foot high, ſhaped like a plug pointing backwards. The bone of this whale is not of much value, tho' ſomewhat better than the fin-back's. His fins are ſometimes 18 foot long and very white; his oil as much in quantity as that of the fin-back. Both the fin-backs and hump-backs are ſhaped in reeves lengthwiſe from head to tail on their bellies and ſides, as far as their fins, which are about half way up their ſides.

The *ſperma ceti* whale is a fiſh much of the ſame dimenſions with the other, but of a greyiſh colour; whereas the others are black: He has a bunch on his back like the hump-back; but then he is diſtinguiſhed by not having any whale-bone in the mouth; inſtead of which there are rows of fine ivory teeth to each jaw, about 5 or 6 inches long. They are a more gentle kind of fiſh than the other whales, and ſeldom fight with their tails; but when ſtruck, uſually turn up their backs and fight with their mouths. The oil, which is made of the body of this fiſh, is much clearer and ſweeter than that of the other whales.

The *ſperma ceti* oil, ſo called, lies in a large trunk about 4 or 5 foot deep, and 10 or 12 foot long, near the whole breadth, depth and length of the head, in the place of the brains, and ſeems to be the ſame, and diſpoſed in ſeveral membranous cells, and not covered with a bone, but a thick griſtly ſubſtance below the ſkin, thro' which they dig a hole and lade out the clear oil; not but that the head, and other glandulous parts of this fiſh will make the *ſperma ceti* oil; but the beſt, and that which is prepared by nature, is in the aforeſaid trunk. And an ingenious perſon, who had himſelf killed ſeveral of theſe whales, aſſured Mr. *Dudley*, that the trunk alone will yield from 10 to 20 barrels: Beſides the *ſperma ceti* oil, this fiſh will yield from 20 to 50 barrels of common oil.

They propagate much like our neat cattle; and therefore, they are called bull, cow and calf. They bring forth but one at a time, and but every other year. When the cow takes bull, ſhe throws herſelf upon her back, ſinking her tail, and ſo the bull ſlides up; and then ſhe claſps him with her fins: A whale's pizzle is 6 foot long, and at the root is 7 or 8 inches in diameter, and tapers away, till it come to about an inch in diameter: His *teſtes* would fill half a barrel; but his genitals are not open or viſible, like thoſe of the true bull. The calf or young whale

whale has been found perfectly formed in the cow, when not above 17 inches long, and white; yet when brought forth, it is uſually 20 feet, but of a black colour; it is ſuppoſed they go with young about 9 or 10 months, and they are very fat in that time, eſpecially, when they bring forth. When the female ſuckles her young, ſhe turns herſelf almoſt upon her back, upon the rim of the water; ſhe has 2 teats of 6 or 8 inches long, and 10 or 12 inches round. The milk is white, like that of a cow; and upon opening a young ſucking whale, the milk was found curdled in his bag, juſt like that of a calf.

Their care of their young is very remarkable; they not only carry them on their tails and ſuckle them; but often riſe with them for the benefit of the air; and however they are chas'd or wounded, yet as long as they have ſenſe, and perceive life in their young, they will never leave them, nor will they then ſtrike with their tail; and if, in their running, the young one loſe his hold and drop off, the dam comes about and paſſing underneath, takes it on again. And therefore, care is taken by ſuch as kill theſe mate fiſh (as they are called) only to faſten the calf; but not to kill her, till they have firſt ſecured the cow. For as ſoon as ever the calf is dead, the cow perceives it and grows ſo violent, that there is no managing her.

The whales are very gregarious, being ſometimes found a hundred in a ſcull, and they are great travellers. In the fall of the year, the whale-bone whales go weſtwards, and in the ſpring they are headed eaſtwards. But here it is to be noted, that the ſeveral kinds of whales do not mix with one another, but keep by themſelves.

Their way of breathing is by two ſpout-holes in the top of the head. The *ſperma ceti* whale has but one, and that on the left ſide of the head. Once in a quarter of an hour, when not diſturbed, they are obſerved to riſe and blow, ſpouting out water and wind, and to draw in freſh air: But when purſued, they will ſometimes keep under water half an hour or more; tho' it be obſerved, when any cow has her calf on her tail, ſhe riſes much oftener for the young one to breathe, without breathing herſelf; out of their breathing holes they ſpout great quantities of blood, when they have received their death wound.

For the firſt year they all ſuck the dam. After they are weaned, the right whales, as is generally ſuppoſed, live upon ſome ouzy matter, which they ſuck up from the bottom of the ſea. The triers, that open them when dead, informed Mr. *Dudley*, that they never obſerved any graſs, fiſh, or any other ſort

of

of food in the right or whale-bone whale; but only a greyiſh ſoft clay, which the people call *Bole Armoniac*; and yet an experienced whale-fiſher told him that he had obſerved this whale in ſtill weather, ſkimming on the ſurface of the water, to take in a ſort of reddiſh ſpawn or brett, as ſome call it, that at ſome times will lie upon the top of the water, for a mile together. Here alſo it may be obſerved, that tho' the body of the whale is ſo very bulky, and ſo exceeding fat; yet when cut open, they are ſeldom found to have much more draught than that of an ox, and they dung much as neat cattle do. Their ſwallow is not much bigger than an ox's; but the fin-back whale has a larger ſwallow; for, he lives upon the ſmaller fiſh, as mackarel, herring, *&c.* great ſculls of which they run thro'; and with a ſhort turn, cauſe an eddy or whirlpool, by the force of which, the ſmall fiſh are brought into a cluſter; ſo that this fiſh, with open mouth, will take in ſome hundreds of them at a time. The *ſperma ceti* whale, beſides other fiſh, feeds much upon a ſmall fiſh, that has a bill; the fiſhermen call them ſquid fiſh. The ſmall pieces of theſe ſquid bills are plainly to be diſcerned in the ambergris, and may be picked out of it; they appear glazy, and reſemble little pieces of broken ſhells.

Mr. *Harris* in his *Bibliotheca Navigantium*, &c. has given a very particular account of the method of taking whales at *Greenland*; and tho' the way in *New England* differ very much from that; yet Mr. *Dudley* waves it; only he takes notice of the boats their whale-men uſe in going from the ſhore after the whale; they are made of cedar clap-boards; and ſo very light, that two men can conveniently carry them; and yet they are 20 foot long, and carry ſix men, *viz.* the harponeer in the fore-part of the boat, four oarmen and the ſteerſman; theſe boats run very ſwift; and by reaſon of their lightneſs can be brought on and off; and ſo kept out of danger: The whale is ſometimes killed with a ſingle ſtroke; and yet at other times ſhe will hold the whale men in play, near half a day together, with their lances; and ſometimes will get away, after they have been lanced and ſpouted blood, with irons in them and drugs faſtened to them, which are thick boards about 14 inches ſquare. The people in *New England* formerly uſed to kill the whales near the ſhore; but now they go off to ſea in ſloops and whale-boats, in *May*, *June* and *July*, between *Cape Cod* and *Bermudas*; where they lie by in the night; and ſail to and again in the day, and ſeldom miſs of them; they bring home the blubber in their ſloops. The true ſeaſon for taking the right or

whale-bone whale is from the beginning of *February* to the end of *May*; for the *ſperma ceti* whale, from the beginning of *June*, to the end of *Auguſt*. And it has been obſerved by the fiſhermen, that when a *ſperma ceti* is ſtruck, he uſually, if not always, throws the excrements out of the *anus*.

The prodigious ſtrength of this animal lies principally in the tail, that being both their offenſive and defenſive weapon. Mr. *Dudley* had ſeveral inſtances of this kind from credible perſons, who were eye witneſſes, a few of which are, as follows; a boat has been cut down from top to bottom with the tail of a whale, as if cut with a ſaw, the clap-boards ſcarce ſplintered, tho' the gunnel upon the top be of tough wood. Another has had the ſtem or ſtern poſt of about three inches thro', and of the toughеſt wood that can be found (into which the ends of the cedar clap-boards are nailed) cut off ſmooth above the cuddee, without ſo much as ſhattering the boat, or drawing the nails of the clap-boards. An oar has been cut off with a ſtroke upwards, and yet not ſo much as lifted up out of the thole-pin. One perſon had an oar cut off, while in his hand; and yet he never felt any jarring.

A few years ſince, one of the fin-back whales came into a harbour near *Cape Cod*, and towed away a ſloop of about 40 tun, out of the harbour into the ſea. This accident happened thus; it was ſuppoſed the whale was rubbing herſelf upon the fluke of the anchor, or going near the bottom got the fluke into her niſket or the orifice of the *uterus*, and finding herſelf caught, tore away with ſuch violence, that ſhe tow'd the ſhip out of the harbour, as faſt as if ſhe had been under ſail with a good gale of wind, to the aſtoniſhment of the people on ſhore; for, there was no body on board. When the whale came into deep water, ſhe went under, and had like to have carried the ſloop with her; but the cable gave away, and ſo the boats that were out after her, recovered her. This whale was found dead ſome days after, upon that ſhore, with the anchor ſticking in her belly.

After a whale has been dead, it has been obſerved, that the ſame way the head lies, ſo the head will lie if not forcibly turned; and let the wind blow which way it will, that way they will ſcull a-head, tho' right in the eye of the wind; and they are much eaſier towed to the ſhore, if they die with their head that way, than any other.

The fiſh that prey upon the whales, and which often kill the young ones, (for they will not venture upon an old one, unleſs much

much wounded) are by the whale-men called killers; these are from 20 to 30 foot long; and have teeth in both jaws that lock within each other. They have a fin, near the middle of their backs, four or five foot long. They go in company by dozens, and set upon a young whale, and will bait him like so many bull-dogs; some will lay hold of his tail to keep him from threshing, while others lay hold of his head, and bite and thresh him; till the poor creature, being thus heated, lolls out his tongue; and then some of the killers catch hold of his lips, and if possible, of his tongue; and after they have killed him, they chiefly feed upon the tongue and head; but when he begins to putrify they leave him. This killer is undoubtedly the *orca*, that Dr. *Frangius* describes in his treatise of animals to the following purpose, 'when an *orca* pursues a whale, the lat-'ter makes a terrible bellowing, like a bull when bit by a dog.' These killers are of such vast strength, that when several boats together have been towing a dead whale, one of them has come and fastened his teeth in her, and carried her away in an instant down to the bottom; and sometimes they have bit out a piece of blubber of about two foot square, which is of that toughness, that an iron with little beards being stuck into it, will hold it, till it draw the boat under water. The killers are sometimes taken and make good oil, but have no whale-bone. The carcases of whales in the sea serve for food for gulls, and other sea-fowl as well as sharks.

Many and various have been the opinions even of the learned world, as to the origin and nature of ambergris: Some have reckoned it a *bitumen*, and to issue from the bowels of the earth; others, that it was produced from some insect, as honey, silk, &c. The famous Mr. *Boyle* communicated in a former *Transaction* an account of ambergris from a *Dutch* merchant, who first denies it to be the scum or excrement of a whale; and then gives it, as his opinion, that it is a fat gum that issues from the root of a tree, and that you may raise it in quantities, by planting those trees by the shore; and so the stream will cast it up to great advantage. But it is now found, that this *occultum naturæ* is an animal production, and bred in the body of the *sperma ceti* whale, analogous to what is found in some land-animals, as the musk-hog or *taiacu*, the musk-deer, the bezoar sheep, and some amphibious animals, as the musquash, &c. who have their valuable scent in a particular *cystis* or bag. Mr. *Dudley* is apt to think, that what first gave occasion to the notion of ambergris being the production of the whale was, be-

cause

cauſe it was found in conſiderable quantities on the ſhore of the *Summer Iſlands* and among the *Bahama*'s, where the dead whales are frequently wreck'd and broke up with the ſea, and the ambergris found floating, or on the ſhore: But here again the ingenious, till very lately, were at a loſs and divided in opinion; for, tho' they agreed that it came from the whale; yet ſome took it to be the true and proper *ſemen*, being found only in the bull, at the root of the *penis*, near the *teſtes*; others again took it to be the ordure or excrement of the whale.

The beſt and moſt exact account Mr. *Dudley* could procure, was from one Mr. *Atkins*, at *Boſton* in *New England*, who uſed the whale fiſhery for 10 or 12 years together; and was one of the firſt, that went out a fiſhing for the *ſperma ceti* whales, about the year 1670, and then he began to diſcover the ambergris; his account, and which agreed with that Mr. *Dudley* had from ſeveral whale-men was, as follows.

'The ambergris is found only in the *ſperma ceti* whales, and 'conſiſts of balls or globular bodies, of various ſizes, from 'about 3 inches to 12 inches in diameter, and will weigh from 'a pound and a half to twenty two pounds, lying looſe in a 'large oval bag or bladder, of three or four foot long, and 'two or three foot deep and wide, almoſt in the form of an ox's 'bladder, only the ends more acute, or like a blackſmith's 'long bellows, with a ſpout running tapering into, and thro' 'the length of the *penis*; and a duct or canal, opening into the 'other end of the bag, and coming from the kidneys; this 'bag lies juſt over the *teſtes*, which are upwards of a foot long, 'and it is placed lengthwiſe at the root of the *penis*, about 'four or five foot below the navel, and three or four foot above 'the *anus*. This bag or bladder is almoſt full of a deep 'orange coloured liquor, not quite ſo thick as oil, and ſmelling 'ſtrong, or rather ſtronger, of the ſame ſcent with the balls of 'ambergris, which float in it; the inſide of the bag is very 'deeply tinged with the ſame colour; the liquor may alſo be 'found in the canal of the *penis*; the balls ſeem to be pretty 'hard, while the whale is alive; inaſmuch as there are fre- 'quently found, upon opening the bag, large concave ſhells, of 'the ſame ſubſtance and conſiſtence, that have ſcaled off from 'them; and the balls themſelves ſeem to be compoſed of ſe- 'veral diſtinct coats, incloſing each other, ſomething like the 'coats of an onion.'

As to the number of balls, Mr. *Atkins* never found above four in a bag; and in a bag where he found one that weighed

21 pounds

21 pounds (which was the largeſt he ever ſaw) there was no other.

Mr. *Atkins* farther affirms, 'that to one *ſperma ceti* whale 'that has any of theſe balls, there are two, that have nothing 'but the aforeſaid deep orange-coloured liquor in their bags.' This remark confirms what another whaleman told Mr. *Dudley*, *viz.* 'that the ambergris was found only in ſuch *ſperma ceti* 'whales, as are old and well grown.'

It is the general opinion of the whalemen, that the ambergris is only produced by the male or bull *ſperma ceti* whale. As to this particular, Mr. *Atkins* affirms, 'he never ſaw nor heard 'of a *ſperma ceti* female taken in his life; the cows of that 'ſpecies of whales being much more timorous than the bulls, 'and almoſt impoſſible to be come at; unleſs they happen to 'be found aſleep on the water, or detained by their calves.' This is certain, that the boats can never come near them, when they are awake, they are ſo very ſhy and timorous.

Mr. *Atkins*'s method of getting the ambergris out of the whale was thus; after the fiſh is killed, he turns the belly upwards and fixes a tackle to the *penis*, then cuts a hole round the root of the *penis*, thro' the rim of the belly, till he come to the intrails; and then ſearching for the duct or canal at the farther end of the bag, he ties it pretty near the bag, and cuts the duct off beyond it; upon which he draws forth the *penis* by the tackle, and the ambergris bag entirely follows it, and comes clean and whole out of the belly.

Mr. *Prince* of *Boſton* (who took the preceeding relation from Mr. *Atkins*) takes the aforeſaid bag to be the urinary bladder, and the ambergris ball to be a certain concretion, formed out of the greaſy odoriferous ſubſtance of the aforeſaid liquor, contained within it.

An enquiry into S. Valſalva's *Diſcovery of an excretory* Duct *from the* Glandula renalis; *by Mr.* John Ranby. Phil. Tranſ. N° 387. p. 270.

MR. *Ranby* procured a human body in order to ſearch for this duct, which he did with all poſſible diligence: He was not ſo happy as to diſcover any duct of this kind; but having injected the *aorta*, he found the arteries, going to the *glandula renales*, diſpoſed, as repreſented in Fig. 11. Plate XIII.

Now whether that branch of the artery, which goes down from the *glandulæ renales*, on both ſides towards the *teſtes*, without ſupplying any of the neighbouring parts, might not

have

have been miſtaken by the learned anatomiſt S. *Valſalva* for an excretory duct (all arteries in dead bodies being generally free from blood, and of a whitiſh colour) Mr. *Ranby* will not determine, for want of farther experiments.

A A (Fig. 11.) repreſents the right kidney; B B the left kidney; C C the deſcending trunk of the *aorta*; D the right emulgent artery; E the left emulgent artery; F the right *glandula renalis*; G G the right ſpermatic artery; H the left *glandula renalis*; I I the left ſpermatic artery; K K the *ureters*; *a a* a ſmall artery ariſing out of the deſcending *aorta*, a little above the right emulgent artery: It ſends two branches *b b* upwards to ſupply the right *glandula renalis*; a third branch *c c* goes downwards towards the right ſpermatic artery G G; and then it farther accompanies the ſame to the right *teſtis*; *d d* a ſmall artery ariſing out of the left emulgent artery, near the deſcending trunk of the *aorta*, and going directly upwards to the left *glandula renalis* H; *e e* a ſmall artery ariſing out of the deſcending trunk of the *aorta*, a little below the left emulgent artery; it divides into two branches; one of which *f f* goes upwards between the emulgent artery and vein to the left *glandula renalis*; the other *g g* downwards, towards the left ſpermatic artery, and accompanies it to the left *teſtis*.

The Sequel *of the* Diſſertation *on the* Figure *of the* Earth; *by Dr.* Deſaguliers. Phil. Tranſ. N° 388. p. 277.

DR. *Deſaguliers* met with a diſſertation of M. *Mairan* (publiſhed in the *Memoirs of the Royal Academy of Paris* for the year 1720) wherein the learned and ingenious author has taken a great deal of pains to reconcile the obſervations made on pendulums (found to be ſhorter at the equator than at *Paris* when they ſwing ſeconds) with the oblong ſpheroidical figure of the earth, deduced from M. *Caſſini*'s meaſures. And tho', upon a ſtrict examination of his conjectures, and what he gives for demonſtrations, Dr. *Deſaguliers* does not find reaſon to alter his opinion, concerning the oblate or flatted ſpheroid, which Sir *Iſaac Newton* has ſhewn to be the figure of the earth: Yet ſince it might be thought by ſome who have read M. *Mairan*'s treatiſe, and afterwards may read the Dr's, that the latter has not conſidered all the circumſtances which the former has done, and that he has not been exact enough in the mathematical part of his diſſertation; becauſe he has drawn ſome concluſions from ſuppoſing the figure of the earth ſpherical, when he

ſhould

ſhould have ſuppos'd it an oblong ſpheroid; the Dr. ſhews here, wherein he thinks M. *Mairan* is miſtaken, and to give additional proofs of his aſſertions in *Phil. Tranſ.* N° 386. 387.

Firſt then the Dr. begins with the conjectures.

M. *Mairan* affirms, that it is as reaſonable to ſuppoſe the earth (if it was once fluid) to have been an oblong ſpheroid at firſt, as a ſphere; and that in ſuch a caſe, the centrifugal force of the ſeveral parts of the earth, ariſing from its revolution about its axis, which might convert a ſphere into an oblate ſpheroid, would only change an oblong ſpheroid into one leſs oblong.

If the earth were at firſt a fluid (ſuppos'd homogeneous and of any given form) and left to thoſe laws, which we find to obtain at preſent, it muſt put on a ſpherical figure, for the ſame reaſon that drops of mercury, of water and other fluids, put on ſuch a figure. And to ſuppoſe any change made in that figure from the preſſure of an external fluid, filling up all ſpace, is contrary to what has been demonſtrated by Sir *Iſaac Newton* in his *Principia lib. 2. prop. 19.* Where he ſhews, 'that if any portion of a fluid be compreſs'd by the 'ſame or any other homogeneous fluid, that portion will not 'have its figure alter'd by that preſſure.'

And indeed, we ſee, that in the receiver of the air-pump, lumps of butter, coagulated oil or honey, drops of quickſilver or water, *&c.* have the ſame figure, whether the preſſure of the air act upon them, or be taken off by exhauſting the receiver.

That a fluid ſubſtance of any figure will by the gravity of its parts become ſpherical, is plain by the following demonſtration.

Let A B C D E (repreſented Fig. 12. Plate XIII.) be a portion of a homogeneous fluid, whoſe parts tend towards each other, and whoſe figure is not ſpherical. If in ſuch a fluid we ſuppoſe a ſyphon, as A C E (or which is the ſame thing, if all the fluid ſhould be frozen, excepting the canal A C E) whoſe legs A C and C E are unequal, and meet at C, the center of the fluid, towards which there is the greateſt tendency: the fluid will run out at A in the leg A C, till it be come down as far as *g* in the leg C E, ſuppoſing C *g* equal to A C. But if the leg A C be lengthened as far as *c*, then the fluid will only come down, as far as *e* in the leg C E, and

at the ſame time riſe up to *a* in the leg C*a*, C*a* being equal to C*e*.

If ſuch another canal or ſyphon be ſuppos'd at BCD, the fluid in it will come down from D to *d*; and riſe from B to *b*. And ſince ſuch ſyphons may be ſuppos'd all over the fluid ABDE, that fluid by the mutual tendency of its parts towards each other, muſt be reduced to the ſpherical figure *abde*. Q. E. D.

Now, without conſidering the unreaſonableneſs of the ſuppoſition, let us imagine the earth to have been an oblong ſpheroid at firſt, and then to have a diurnal rotation given it, which ſhould by degrees ſhorten its axis, to bring it to what M. *Caſſini* and M. *Mairan* ſuppoſe it at preſent to be. If in ſuch a caſe the earth be ſuppoſed fluid enough to change its figure, by the rotation round its axis, why ſhould it ſtop when the equatorial diameter comes to want $\frac{1}{96}$ part of the length of the axis? Since two powers, *viz.* gravity and the centrifugal force act upon it to ſhorten its axis; the firſt of theſe has already been ſhewn capable to reduce it to a ſphere, and the centrifugal force is acknowledged by M. *Mairan* to be (as Sir *Iſaac Newton* has prov'd it) equal at the equator to $\frac{1}{289}$ part of the gravity there. Certainly the alteration of figure would not have ſtopp'd, before the earth came to be a ſphere; nay, and it muſt have riſen at the equator; and how much, Dr. *Deſaguliers* has already ſhewn in *Phil. Tranſ.* N° 386. 387.

Again, if we ſuppoſe the earth compos'd of a heterogeneous fluid, before the diurnal revolution, the heavieſt parts would tend towards the center, and the lighter towards the ſurface, and that way the terraqueous globe would alſo become a ſphere. Then if, when the central forces are fix'd, and the ſuperficial *ſtrata* fluid, the earth receive a diurnal motion; it will riſe at the equatorial parts, and that to a greater height than what the Dr. has ſhewn in the ſaid *Tranſactions*, where he ſuppos'd the earth, compos'd of uniform matter. And that ſomething like this muſt be the caſe, appears from what Sir *Iſaac Newton* has ſaid upon this ſubject. For, after having ſhewn, from ſuppoſing the earth, compos'd of uniform matter, that the centrifugal force of all its parts would bring it to be 17 $\frac{1}{6}$ *Engliſh* miles higher at the equator than at the poles, and after having given a table of the proportionable decreaſe of the length of the degrees of a

meridian

meridian of the earth, going from the poles to the equator in such a figure of the earth with the lengths that pendulum's must have to swing seconds in several latitudes; from a comparison of the lengths of pendulums (observ'd by different persons to be shorter towards the equator, than in greater latitudes (when they swing seconds) he shews, that the earth must be 31 $\frac{1}{2}$ miles higher at the equator than at the poles; and therefore, that it must be denser towards the central than the superficial parts to produce a flatted spheroid, where the equatorial diameter must exceed the axis so much more, that is, be longer something more than $\frac{1}{230}$ part.

Dr. *Desaguliers* is very well aware, that it may be objected by such as have read M. *Mairan*'s dissertation, and have not read Sir *Isaac Newton*'s *Principia*, or not with due attention, that the Dr. has not argu'd fairly, in drawing consequences from a greater gravity at the equator than at the poles, in an oblong spheroid; because M. *Mairan* has shewn, that in such a figure of the earth, the gravity is greater at the poles than at the equator; and that the Dr. should have drawn his consequences from these principles. To this the Dr. answers, that M. *Mairan*'s demonstrations about gravity are built upon wrong suppositions, as shall be shewn anon. Nevertheless, supposing gravity to be greater towards the poles than towards the equator, in the proportion he assigns; namely of the ray of curvature, drawn into the perpendicular to the curve, terminated at the axis; let us consider what will follow from his principles.

Let us then suppose the earth at first in a fluid state; A A (as represented Fig. 13.) the axis; *d* Æ the equatorial diameter; *a b* a ray of curvature; *d n* another; *a c* and *d* C two lines of tendency, or perpendiculars to the curve, intercepted by the axis at *c* and C; and *d* C, A C two tubes or canals of the fluid, gravitating towards and communicating at C; the Dr. says, that according to M. *Mairan*'s principles of gravity, the earth cannot retain its oblong spheroidical figure: For, since the gravity at *a*: is to the gravity at *d*: :: as $dn \times dC$: to $ab \times ac$; it will follow (from the nature of the ellipsis) that the gravity at A: will be to the gravity at *d*: : as AC^4: to dC^4: and therefore, the forces, with which the columns of fluid A C and *d* C tend towards C, will be as their masses, drawn into the forces, driving towards C, that is, as $AC \times AC^4$ to $dC \times dC^4$. Now by the principles of hydrostatics, it is evident, that the fluid, in the canal A C, will cause the fluid in

the canal dC to run out at d, as long as AC × AC^4 is greater than dC × dC^4; and if the canal Cd be continued quite to δ, the ſurface of the fluid in AC will ſink to α, whilſt the ſurface of the fluid in dC riſes up to δ; in which caſe as αC = Cδ, the point A will come to α, and the point d to δ, and the curve Ad being changed into $\alpha\delta$, the oblong ſpheroid will be changed into a ſphere, the only figure conſiſtent with the *equilibrium* of the fluid parts, according to M. *Mairan*'s own principles; becauſe then you will have AC^4 = dC^4, and AC × AC^4 = dC × dC^4. If we make uſe of Sir *Iſaac Newton*'s principles in this reaſoning, we ſhall alſo ſhew, that an oblong, ſpheroidical, fluid earth will be changed into a ſphere; but not ſo faſt, as it does by M. *Mairan*'s laws: For, according to Sir *Iſaac Newton*, the gravity at A: is to the gravity at d: : as $\sqrt[5]{Cd} : \sqrt[5]{AC}$. Q. E. D.

Here we have ſuppos'd no diurnal revolution; for, as ſoon as that begins, the centrifugal force will raiſe the equatorial parts, and change the ſphere into a flatted ſpheroid; as has been ſhewn before, and is allow'd by M. *Mairan*.

Now, if we ſuppoſe the ſame figure of the earth, but the land (at its firſt creation) as firm as it is now; it will in that caſe follow from M. *Mairan*'s principles, that the ſea muſt riſe and overflow all the equatorial regions, tho' the earth had no diurnal revolution; and much more ſo, when the centrifugal force, ariſing from the diurnal motion, helps to carry the water the ſame way.

Demonſtration. Let P æ P Æ Fig. 14. repreſent the plane of a meridian; P P the axis of the earth (ſuppos'd an oblong ſpheroid) æ Æ the diameter of the equator; d e a æ part of the ſurface of the earth; æ A and eB two perpendiculars to the ſurface of the earth (which are here two rays of curvature) fc the ſurface of the ſea, and f d e g, b a æ c two cylinders of ſea water of equal baſes and equal heights.

Since gravity acts on the unequal columns of water b a c æ; f d e g in the reciprocal ratio of the ray of curvature (at the reſpective places of the columns) drawn into that part of it, which M. *Mairan* calls the line of tendency (that is, in the ratio of eB × eZ to æ A × æ C) the weight of fe: will be to the weight of bæ: : as æ A × æ C : eB × eZ; therefore, if there be a communication between the fluid columns fe and bæ, there cannot be an *equilibrium*, till the quantity of matter in fe be to the quantity of matter in bæ, reciprocally

as

as the gravity at the place *æ* is to the gravity at *e*; and in that caſe the height *ge* will be reduced to *ke*; if *ke* : *cæ* : : *e*B × *e*Z : *æ*A × *æ*C; and conſequently, the ſurface of the ſea will go thro' the points *ikbc*, where *bc* under the equator is higher than *ik* towards the poles, Q. E. D.

N. B. That the centrifugal force will ſtill add to the height of the ſea at *bc*, is plain from what was ſaid before. And if we apply theſe principles to determine the different lengths of pendulum's, ſwinging ſeconds at *Paris* and at the equator; from the gravity at *Paris*, compared with the gravity at the equator (in this ſuppoſition of the action of gravity and figure of the earth) a pendulum muſt be ſhorter at the equator by more than 10 lines, without conſidering the centrifugal force and if the centrifugal force be taken into the account, the pendulum's muſt be ſhortened near a whole inch. But this being about five times more than what agrees with obſervation; what proves too much, proves nothing at all.

Having thus ſhewn, that M. *Mairan*'s account of the action of gravity, on ſeveral places upon the ſurface of the earth, can be of no ſervice for reconciling the experiments made on pendulum's, with the figure of the earth, deduced from M. *Caſſini*'s meaſures; Dr. *Deſaguliers* proceeds to ſhew, that his demonſtrations are founded upon wrong principles; and firſt in relation to gravity.

This Gentleman has followed Sir *Iſaac Newton* in ſaying, that gravity increaſes in a duplicate reciprocal ratio of the diminiſhed diſtance from the center of the force, and ſo *vice verſâ*; but he has followed Sir *Iſaac Newton* no farther than ſerved his preſent purpoſe; otherwiſe he would have known — That with reſpect to a central body, as a planet towards which others are attracted or impelled by gravity, this law obtains only, as bodies attracted are removed from the ſurface of the planet, to greater diſtances from the center, compared with that diſtance, or as from greater diſtances they approach nearer to the planet — That the greateſt action of gravity is at the ſurface of the planet — That afterwards in advancing towards the center, the force of gravity on the body attracted, becomes continually leſs, decreaſing directly as the diſtance; and that this holds true in a ſpheroid as well as in a ſphere — That on different parts of the ſurface of the earth (in the condition it is now) the gravity on bodies is reciprocally, as their diſtance from the center of the earth — That tho' at a conſiderable diſtance we look upon the earth, or any planet, or even the ſun,

as

as a point (in the center of the forces tending towards it) endued with an abſolute force, proportional to its quantity of matter; yet when we come ſo near the body, as to conſider the ſpace it poſſeſſes, we are to take notice, that the whole attraction or gravity of the body is made up of the ſum of the attraction of all its parts, properly combined; and therefore, that when a corpuſcle, or body attracted, comes to be within the planet or body attracting, the matter above it draws it back in ſuch a manner, that it leaves it only a force to go on towards the center, which is directly as the diſtance, as has been ſaid already: Juſt as if a body concentric to the planet (whether ſpherical or ſpheroidal) had its ſurface juſt where the corpuſcle is, and all the exterior cruſt or ſhell were annihilated.

The Dr. does not doubt, but M. *Mairan* will be of this opinion, when he has carefully and impartially examined the 12th and 13th ſections of Sir *Iſaac Newton's Principia*, and the 18th, 19th and 20th *Prop.* of the third book. And if he will be at the pains to compare the 38th and 39th *propoſitions* of the third book with the 66th of the firſt, he will find, that the preceſſion of the equinoxes is owing to the broad ſpheroidical figure of the earth; and that if it had M. *Caſſini's* figure, the equinoctial points would move *in conſequentia* faſter than they do now *in antecedentia*.

Farther M. *Mairan* demonſtrates, that in an oblong ſpheroid, the diminution of gravity by the centrifugal force, increaſes faſter in going from the poles to the equator, than it would do in a broad ſpheroid; and therefore would ſhew, 'that notwithſtanding the ſurface of the earth is nearer to the center in M. *Caſſini's* figure than in Sir *Iſaac Newton's*; yet the centrifugal force will diminiſh the gravity ſo faſt in going from *Paris* to the equator, that the ſhortening of pendulums, to make them ſwing ſeconds at the equator, may very well be accounted for that way.'

Now let us examine into this matter, to ſee whether the cauſe be adequate to the effect.

If the diſtance from the ſurface of the earth at the pole to the center be 96, and the diſtance of the ſurface at the equator be 95, the diſtance of the ſurface at *Paris*, in the Lat. of 48° 50', will (by the property of the ellipſis) be 95,562, *&c.* Now ſince the force of gravity, in different places on the earth's ſurface, is reciprocally as the diſtance from the center; and the lengths of pendulum's, that perform their vibrations in the ſame time, are directly as the force of gravity; therefore, the length

length of pendulums at *Paris* will be to their length at the equator, as 95 to 95,562, &c. that is, as 440,555, &c. to 443,165, &c. and consequently, they must be lengthened 2, 61, &c. lines. But as from M. *Mairan*'s principles, the diminution of gravity by the centrifugal force, is greater at the equator than at *Paris*, hardly $\frac{1}{240}$ part of the whole gravity at the equator, the pendulum's must be shortened in that proportion; so that then the length of second-pendulum's will be 440,555 + 2,61 — 1 lines. But as that quantity is greater than 440,555, &c. therefore, the pendulum's upon the whole must be lengthened: Nay, tho' we should allow a shortening of two lines; since by observation pendulum's are found to be about two lines shorter at the equator, the oblong spheroidical figure of the earth cannot be consistent with the experiments on pendulum's.

Dr. *Desaguliers* here sets down M. *Mairan*'s aforesaid demonstration, in order to see, whether he has assumed true principles, vide M. *Mairan*'s dissertation, article 11, &c.

Propos. 5. 'The centrifugal force at any degree of latitude, 'taken upon the oblong spheroid, between the equator and the 'pole, is less in comparison to the centrifugal force at the equator, than it would be at the same degree of latitude, taken 'upon a sphere, or which is the same thing, the centrifugal 'force increases more, going from the poles towards the equator, 'upon an oblong spheroid, than upon a perfect sphere; and 'consequently, gravity diminishes more, and a pendulum must 'be more shortened under the equator, in the hypothesis of the 'oblong spheroid, than in that of a perfect sphere.

'Having described an oval curve of any kind, as for example, 'the abovementioned ellipsis A D B E (Fig. 15.) and inscribed 'the circle D H E, whose radius is D C = half the shorter axis 'D E; upon A D take any point as R, between the equator and 'pole; and from that point to the *evoluta* O T X draw the ray 'of curvature R T, which gives the line of tendency R P '(Art. 4.) likewise draw from the common center C, to the 'circumference of the circle D H; a radius C V, parallel to 'P R, and meeting the circle at V; then from the points R V, 'draw the lines R N, V Z, perpendicular to the axis A B.

'It must be observed, first, that as the ellipsis A D represents 'a meridian of the oblong spheroid, the circle D H represents 'a meridian of a sphere in the same plane.

Secondly,

'Secondly, that the point V on the circular meridian, anſwers to the ſame degree of latitude, as the point R upon the elliptical meridian; becauſe the lines P R, C V, being parallel to each other, and perpendicular, the one to the ellipſis and the other to the circle (by conſtruction) the touching planes, or horizons of the points R V, will likewiſe be parallel.

'Thirdly, hence it follows, that the diminution of the centrifugal force (acting againſt gravity) on account of its obliquity to the horizon (Art. 10.) of the ſame degree of latitude on the elliptical and on the circular meridian, is the ſame in both caſes, and in the ſame ratio, as the abſolute centrifugal forces, repreſented by the perpendiculars R N, V Z (Art. 9.) therefore, to know whether the centrifugal force (either abſolute or relative) of the point R, upon the oblong ſpheroid A D B E be leſs or greater with reſpect to the centrifugal force under the common equator D E, than the centrifugal force (whether abſolute or relative) of the correſpondent point V upon the ſphere; nothing more is required than to ſee, which is the longeſt of the two perpendiculars, namely, R N in the oblong ſpheroid, or V Z in the ſphere: Since theſe two lines expreſs the *radii* of the circles of revolution; and conſequently, the abſolute quantity of the centrifugal forces.

'Fourthly and laſtly, that the ratio of the centrifugal forces of two correſpondent points upon the oblong ſpheroid A D B E and the inſcribed ſphere D H E, to the centrifugal force of their equators is the ſame, ſuppoſing the ſphere of any other bigneſs; and that it has been determined here of the diameter D E, only to render the demonſtration eaſier, by giving the ſame conſequent to the antecedents R N and V Z. For, if about the center C and with the radius C *d*, the circle *d h e* be deſcribed equal, for example, to a meridian of a ſphere of the ſame ſolidity, as the oblong ſpheroid A D B E; and the radius C V be produced, till it meet the circle *d h* at the point *u*, and *u z* be let fall perpendicular to the common axis of revolution, and parallel to V Z. It is plain, that we ſhall always have V Z : D C : : *u z* : *d* C; or $\frac{VZ}{DC} = \frac{uz}{dC}$; and conſequently, $\frac{RN}{DC}$ will have the ſame ratio to $\frac{UZ}{DC}$ as to $\frac{uz}{dC}$.

'Therefore, in order to demonſtrate, that the centrifugal force of a point, taken in any latitude upon the oblong ſpheroid is leſs when compared to the centrifugal force of the like

'point,

'point, taken upon a ſphere, with reſpect to the centrifugal 'force at the equator; there is nothing more required than to 'ſhew that $RN < VZ$; becauſe by that means we ſhall have $\frac{RN}{DC} < \frac{VZ}{DC}$.

'This being obſerved; from the point R, draw the line RI, 'parallel to the axis AB, and meeting the circle DH at K, 'and the diameter DE of the equator at the point I: From 'the point K having let fall the perpendicular $KL = RN$, 'upon the axis AB, and drawn KC to the center C; the queſ- 'tion will be brought to this, *viz.* to know whether the point 'V coincides with the point K; or whether it is above it to- 'wards D, or below towards H.

'But $CK = CV = CD > PR$ (Art. 8.) therefore, CK 'and PR being both between the parallels AC, RI, the 'greateſt CK is more inclined to them than the leaſt PR; and 'the angle KCA is leſs than the angle $RPA = VCA$: And ſince 'theſe two angles have each of them one of their ſides, coin- 'ciding with the line AC, namely, the ſide AP of the angle 'RPA, and the ſide AC of the angle KCA; it follows, that 'the ſide VC of the angle $VCA = RPA > KCA$, will go 'above CK between CK and CD, and meet the line RI at 'the point G, between K and I, and the circle DH at the 'point V, which conſequently will be above RI, between K 'and D: Therefore, $CV = CG + GV$ is $= PR + GV$; 'and conſequently, VZ, which meets RI at the point F, is '$= ZF + FV = RN + FV$; and therefore, $RN = VZ$ '$- FV$; therefore $RN < VZ$.

'And becauſe the ſame thing may be demonſtrated with re- 'ſpect to any other point, taken between the equator and the 'pole; and that gravity, and conſequently, the length of a 'pendulum diminiſhes, as the centrifugal force increaſes. 'Therefore, *&c.* Q. E. D.

'*Corollary* 12. From what has been demonſtrated, and 'from *Prop.* 3. Art. 8. it follows, that the perpendicular, 'which is drawn from any point of an oval meridian to the 'axis, will be ſo much ſhorter, in compariſon to the perpendicu- 'lar drawn from the correſpondent point of an inſcribed circular 'meridian, as the latitude is greater; and conſequently (by 'Art. 11. N° 3.) the centrifugal force will be ſo much the leſs, 'and gravity ſo much the greater, upon the oblong ſpheroid, 'with reſpect to the centrifugal force, and the gravity under the 'equator.

'For, as the line R P does always decreaſe, as the point R 'is taken nearer the pole A; it is evident, that the angle V C K 'will continually increaſe, with reſpect to the angles V C A, 'K C A, as it is their difference; and conſequently, that the 'perpendicular V Z will be ſo much greater than the perpendi-'cular K L = R N.'

The Dr. omits the demonſtration of the latter part of M. *Mairan*'s abovementioned propoſition, which he juſtly deduced from his conſtruction, if what he ſays (N° 2.) be right; becauſe in ſuch a caſe it cannot be called in queſtion; and proceeds to an obſervation he makes afterwards, *viz.* 'We muſt take care to 'obſerve in the foregoing propoſitions and corollaries, that the 'compariſon is always made between two ſimilar points of lati-'tude, taken upon the two ſpheroids, or upon one of the ſphe-'roids and the ſphere between the equator and the poles, with 'reſpect to the centrifugal force upon the equator of any one of 'theſe ſpheroids or of the ſphere. For, if we only compared 'abſolutely the centrifugal force of a point of the equator of 'the one, to the centrifugal force of a correſponent point of the 'equator of the other; it is plain, that it would be greater upon 'a flatted ſpheroid than upon a ſphere, or than upon an oblong 'ſpheroid of the ſame ſolidity, in the ratio of the great axis of the 'generating ellipſis of the flatted ſpheroid, to the diameter of the 'ſphere, or to the ſhorter axis of the generating ellipſis of the 'oblong ſpheroid. And in all likelihood, this muſt be the rea-'ſon that has made others, who have treated of this ſubject to 'imagine the very contrary of what I have demonſtrated.'

As M. *Mairan* conſiders the earth at reſt, in the conſtruction for his above quoted demonſtration, and afterwards obſerves what effect the centrifugal force will have upon bodies on its ſurface, to diminiſh the gravity, with which they endeavour to deſcend in their line of tendency R P: He ſhould not only have taken notice (as he has done) that the whole centrifugal force N R is not to be ſubſtracted from the gravity at R, as the whole centrifugal force C D is to be ſubſtracted from the whole gravity at D, becauſe of the obliquity of R N to P R; but he ſhould have alſo obſerved, that the obliquity of the plane of the parallel N R, in which the centrifugal force acts, muſt alter the line of tendency R P, and change the direction R P into R W, ſomewhere between the point P and the center C; for, if there be a heavy body as a plummet, hanging by a thread in the line S R or S P, the line of tendency which has been ſuppoſed perpendicular to the curve A R D, without taking in the effect of the centrifugal force; as ſoon as the ſpheroid revolves about its

axis,

axis, the body which would fall in the line S R, acted upon only by one force, namely, that of gravity, will now be acted upon by another force, at the same time pushing it in the line S *s* (which is the same as R *r*) and consequently will move in the line S *r*, diagonal of the parallelogram *s* S R *r*; or which is all one, a body placed at R will have its line of tendency in R W, as the Dr. has already shewn in *Phil. Transf.* N° 386; only he did not suppose the earth a spheroid before the diurnal motion; and therefore, made use of the line Z V, instead of the line N R: So that it may be objected, that the angle *r* S R will not be so great in a spheroid, as in a sphere; because the centrifugal force, which acts with the same obliquity (since N R P = Z V C) is as much less in the spheroid, as N R is less than Z V. But the Dr. was aware of this; and therefore, he made the angle R S *r* only of five minutes, when it really appears to be almost six, when the earth is supposed spherical; and therefore, without coming to give the exact quantity of the said angle, one may easily perceive, that M. *Cassini*'s difference of the axis and equatorial diameter will produce a figure, in which the angle R S *r* will not be less than five minutes.

Such an obliquity, caused in the direction of gravity, will render the oblong spheroidical figure of the earth impossible; because then fluids would not have the lines of their gravity, perpendicular to the horizons of the places, where they are (supposing the horizons of places to be planes, touching the curve of the earth in those places) and plumb-lines would be so far out of the perpendicular to lines of level, as to make an angle easy to be observed, as the Dr. has shewn before.

But if the same cause be supposed to act upon the sea to make it level, as makes heavy bodies to fall (which certainly must) then indeed, lines of level will be perpendicular to plumb-lines, and the level of the sea, taken always for the horizon of a place, will not be a plane touching the earth, but cutting it towards the poles, and consequently, the water will be carried towards the equator, as was before shewn.

Besides, the difference of the action of the centrifugal force would not be so great between correspondent points of the same latitude in the spheroid and in the sphere: For, when the line of tendency R P is by the centrifugal force changed into R W, the point R upon the spheroid does no longer correspond in latitude with the point V upon the sphere; but must be taken nearer to V; so that the line R W may become parallel to V C, and R W A = V C A.

If it be alledged here, that M. *Mairan* ſuppoſes the earth in motion, and takes in the effect of the centrifugal force, when he makes the line of tendency to be R P; to this Dr. *Deſaguliers* anſwers, that if M. *Mairan* had conſidered the earth, as revolving upon its axis, he would not have made V C, the line of tendency of a ſpherical earth in motion; ſince it is the line of tendency of ſuch an earth at reſt.

In M. *Mairan*'s obſervation abovementioned, he ſays, 'that 'we are not to compare the centrifugal force at the equator of 'an oblong ſpheroid, with the centrifugal force at the equator 'of a ſphere, or at the equator of a flatted ſpheroid of the 'ſame ſolidity; allowing that then it would be greater in the 'ſphere, and ſtill greater in the flatted ſpheroid: But only the 'centrifugal forces in ſeveral latitudes upon the ſame figure.' Dr. *Deſaguliers* differs from him for the following reaſons.

1. Becauſe the force of gravity is not the ſame at the equator of the flatted ſpheroid, as it is at the equator of the ſphere, or as it is at the equator of the oblong ſpheroid.

2. Becauſe it is not the ſame in different latitudes, in either of the ſpheroids (vide Sir *Iſaac Newton's Princip. lib.* 3. *Prop.* 19, 20.) And M. *Mairan*'s way of arguing will only ſerve, in caſe the gravity ſhould be the ſame in all the points of the ſurface of the earth in his figure, and alſo in the other two figures.

For example, let the uniform gravity be called g; and

1. Let the centrifugal force at the equator of the flatted ſpheroid be called $c + 2$, and the centrifugal force in any latitude, as for inſtance, the latitude of *Paris* (as it is diminiſhed on account of a ſhorter co ſine of latitude, and likewiſe on account of its obliquity to the line of tendency) be called $c + 2 - l$ the difference of the diminution of gravity at *Paris*, and at the equator will be $\overline{g - \overline{c + 2}} - \overline{g - \overline{c + 2 - l}} = l$.

2. Let the centrifugal force at the equator of the ſphere be called $c + 1$, and the centrifugal force at the latitude of *Paris* be called $c + 1 - \overline{l + m}$; the difference of the diminution of gravity at *Paris*, and at the equator in a ſpherical earth, will be $\overline{g - \overline{c + 1}} - \overline{g - \overline{c + 1 - \overline{l + m}}} = l + m$.

3. Let the centrifugal force at the equator of the oblong ſpheroid be called c, and the centrifugal force at *Paris* be called $c - \overline{l + m + n}$, the difference of the diminution of gravity

gravity at *Paris*, and at the equator, in an oblong ſpheroidical earth, will be $g - c - g - c - l + m + n = l + m + n$.

Now, if gravity ſhould in every caſe be equal to g; it is evident, that the ſhortening of pendulum's at the equator would be greater in the oblong ſpheroid than in the ſphere, or in the flatted ſpheroid; becauſe as the lengths of pendulum's diminiſh with the gravity, thoſe lengths will be at *Paris* and at the equator, when compared, as $g - c + 2 - l$ to $g - c + 2$, in the flatted ſpheroid, as $g - c + 1 - l + m$ to $g - c + 1$ in the ſphere; and as $g - c - l + m + n$ to $g - c$ in the oblong ſpheroid; and conſequently, from what M. *Mairan* has demonſtrated, this ratio of $g - c - l + m + n$ to $g - c$, being greater than either of the others, the pendulum's muſt be ſhortened in the oblong ſpheroid.

But as the force of gravity is leſs at the equator of the flatted ſpheroid, than at the equator of the ſphere, or of the oblong ſpheroid of the ſame ſolidity; let us expreſs its quantity in the three caſes by $g - s$, g, and $g + s$, and we ſhall then find the lengths of the pendulums, at the equator of the three ſolids, as $g - s - c + 2$, $g - c + 1$, and $g + s - c$; and conſequently, the lengths of pendulum's will be greateſt at the equator of the oblong ſpheroid, becauſe $g + s - c$ is the greateſt quantity.

Laſtly, to compare the lengths of pendulum's at the equator of the oblong ſpheroid, thus found, with their lengths at the latitude of *Paris* upon the ſaid ſpheroid. Let us expreſs the exceſs of gravity at the equator, whereby it is greater than at *Paris* (becauſe in this figure, *Paris* is farther from the center of the earth than the equator by $\frac{1}{190}$ part) by the letter s, and the exceſs of the centrifugal force at the equator, above that part of it which acts directly againſt gravity at *Paris*, by $l + m + n$, the gravity at *Paris* by g, and the centrifugal force at the equator by c; then $g + s - c$ will ſtill repreſent the diminiſhed gravity, and anſwer to the length of pendulum's at the equator, whilſt $g - c - l + m + n$, or $g - c + l + m + n$ repreſents the diminiſh'd gravity; and conſequently the length of pendulum's at *Paris*. If s be equal to $l + m + n$, pendulum's will be as long at the equator, as at *Paris*; and if s be greater than $l + m + n$, pendulum's will be longer at the equa-

equator. But making all poſſible allowance, in favour of M. *Mairan*'s hypotheſis, no calculation will bring $l + m + n$ to be greater than, or ever equal to s: Therefore, his demonſtrations, abovementioned, are of no force to prove the earth an oblong ſpheroid.

And now, Dr. *Deſaguliers* thinks, he has anſwered all that relates to the figure of the earth in M. *Mairan*'s diſſertation; by ſhewing, that his conjectures can neither be ſupported by thoſe phyſical principles, which Sir *Iſaac Newton* has mathematically deduced from unqueſtioned obſervations and experiments, accurately made; nor even by thoſe principles M. *Mairan* himſelf has aſſumed to ſerve his intended purpoſe — That his demonſtrations, relating to the difference of the action of the centrifugal force, are of no ſervice to him, for reconciling the experiments made on pendulum's, with M. *Caſſini*'s meaſures; becauſe when applied to Sir *Iſaac Newton*'s principles, they will make pendulum's longer at the equator than at *Paris*; and when applied to M. *Mairan*'s own principles, they will make them a whole inch ſhorter at the equator than at *Paris*, contrary to all obſervations, which at a medium make pendulum's, but about two lines or $\frac{166}{1000}$ part of an inch longer at the equator than at *Paris* — That he has built his demonſtrations upon a wrong notion of gravity — And that he has not conſidered what is moſt material in the effect of the centrifugal force, acting on bodies deſcending by their gravity, between the equator and the poles; namely, the alteration of their line of direction, which would make them fall out of the perpendicular towards the equator.

Dr. *Deſaguliers* adds one more philoſophical argument, communicated to him by a friend, and which is entirely independent on thoſe principles of philoſophy, concerning which, ſome of the Gentlemen that believe the oblong ſpheroidical figure of the earth and the *Engliſh* philoſophers, are not yet agreed; and it is this.

If the earth were of an oblong ſpheroidical figure, higher at the poles than at the equator; the axis of its revolution would either go thro' one of its ſhort diameters, or be continually changing, unleſs the axis did exactly coincide with the axis of the figure.

Demonſtration. Suppoſe ſuch an oblong figure as A a (Fig. 16.) fixed to the axis P p at the center C, but capable of moving freely round it towards P or towards p; yet ſo as to be obliged to move with the axis, when it is turned round. Suppoſe now the poles P and p to be fixed; and the body, thus

con

conſtituted, to be turned ſwiftly round the axis P*p*; then if the angle ACP be oblique, and the figure AD*a*E be oblong, the parts AC and C*a* will acquire a centrifugal force, which will enlarge the angle *p*CA, till it comes to be a right one. Beſides this, a velocity will be generated in the motion, while A is going towards the perpendicular *a*C, which will make it go farther on towards P, as to B, with a motion which will after that be retarded, till the centrifugal force has ſtrength enough to ſend it back again the contrary way; and ſo it will move continually with a reciprocal motion, like the oſcillation of a pendulum; and if a little of this motion be loſt at every oſcillation, then the oblong figure AD*a*E will at laſt move quietly about its leſſer axis DE, coinciding with P*p*.

If A*a* did not at firſt exactly coincide at P*p*, the centrifugal force will have the above-mentioned effect; and that this is not the caſe in the earth is more than probable; becauſe the unequal diſtribution of ſea and land, beſides the phænomena of the tides muſt make the axis of its gravity, and conſequently, the axis of its revolution, to differ from the axis of the oblong ſpheroid, if the earth had ſuch a figure; without conſidering that every earthquake would alter ſo nice an *equilibrium*, which once loſt would never be recover'd again.

To leave nothing unexamined, relating to the controverſy, Dr. *Deſaguliers* again conſiders the meaſures and obſervations mentioned in the account of the meridian, drawn thro' *France*, in the *Memoirs of the Royal Academy*, for the year 1720; and he finds them to want a great deal of the accuracy, requir'd in ſo nice a point, as determining the different lengths of degrees upon the ſurface of the earth: To prove his aſſertion, Dr. *Deſaguliers* refers the reader to the following tables, by which it appears, that if any thing certain can be deduced from the ſaid obſervations and meaſures (either taken as they are, or reduced to the level of the ſea, by M. *Caſſini*'s rules, vide *Memoirs of the Royal Academy* for the year 1720 *vol.* I. *p.* 1. *ch.* 13.) it will be in favour of Sir *Iſaac Newton*'s figure of the earth, rather than theirs.

In the following table, the firſt column exhibits the names of places; the ſecond the diſtances from *Paris*, according to the meaſures taken by the *French* gentlemen; the third the latitudes, ſuch as the meaſured diſtances will give them, ſuppoſing the earth ſpherical; the fifth the differences between

between theſe and the latitudes obſerv'd, expreſs'd in ſeconds of a degree, where, when the latitude computed exceeds the latitude obſerv'd, the letter N (north) ſhews that difference to be in favour of M. *Caſſini*'s figure, and the contrary difference, mark'd by the letter S (ſouth) is in favour of Sir *Iſaac Newton*'s figure.

Names of Places.	Diſtances from *Paris* meaſured.	Latitudes obſerved.			Latitudes in a ſpherical Earth computed from the meaſured diſtances.			Differences in ſeconds.	
I.	II. Toiſes.	III.			IV.			V.	
		°	′	″	°	′	″	″	
Dunkirk.	125552	51	2	25 ½	51	2	25 ½	0	
Amiens.	60370	49	53	48	49	53	48,	0	
Sourdon.	49970 ½	49	42	42	49	42	52,1	10,1	N
Paris.		48	50	10	48	50	20,3	10,3	N
Malvoiſine	18838	48	30	47	48	30	32,1	14,9	S
Vouſon.	67962	47	39	17	47	38	53,6	23,4	S
Bourges.	100192	47	4	31	47	04	58,7	27,7	N
S. Sauvier	139934	46	23	24	46	23	12	12,0	S
Croc.	169540	45	51	43	45	52	4,6	21,6	N
Bort.	196484	45	23	27	45	23	45,2	18,2	N
Aurillac.	223606	44	55	13	44	55	14,5	1,5	N
Rodès.	256575	44	20	54	44	20	35,1	19,9	S
Alby.	280612	43	55	32	43	55	19	13,0	S
Carcaſſone	321430	43	12	56	43	12	24,5	31,5	S
Collioure.	360604	42	31	13 ⅔	42	31	13,8	0.	

In this table it is to be obſerved, that there is an equal number of differences, marked N. and S. and if the differences on each ſide be added together, there will be 89″ 4′ on the north ſide, and 114″,7 on the ſouth. This laſt agrees beſt with Sir *Iſaac Newton*'s figure, which muſt be ſuppoſed for the correction of ſo great a difference.

In the next table, the firſt column exhibits the names of places; the ſecond the latitudes obſerved; the third the diſtances in the meridian from *Paris*, reduced to the level of the ſea; the fourth the differences of the ſecond column, expreſſed in ſeconds of a degree; the fifth the differences of the numbers in the third column; and the ſixth the meaſure of a degree by the fourth and fifth columns compared.

Names

I. Names of places.	II.			III.	IV. Second of a degree.	V.	VI.
				Toises.		Toises.	Toises.
Dunkirk.	51°	2′	19′	125454	4103″	65010	57040
Amiens.	49	53	56	60444	1859	29416	56965
Clermont.	49	22	57	31028	1967	31028	56787
The R. Obſervatory.	48	50	10	0	4253	67959	57525
Voulon.	47	39	17	67959	4553	71978	56912
S. Sauvier.	46	23	24	139237	1901	29602	56058
Croc.	45	51	43	169539	1677	26941	57834
Bort.	45	23	46	196480	1713	27136	57028
Aurillac.	44	55	13	223616	2060	32858	57422
Rodés.	44	20	53	256474	1521	24138	57131
Alby.	43	55	32	280612	2557	40818	57468
Carcaſſone.	43	12	55	321430	2502	39184	56380
Collioure.	42	31	13	360614			

In this table in the third column, over againſt St. *Sauvier*, the number which was 139944 is corrected to make it 139937, in favour of the oblong ſpheroid. In the ſixth column, the numbers appear ſo irregular, as to be unfit to decide this controverſy. Then if a compariſon be made between *Dunkirk*, St. *Sauvier* (which is very near the middle of *France*, and almoſt in the meridian of *Paris*) and *Collioure*, the menſuration is abſolutely in favour of Sir *Iſaac Newton*'s theory; the mean degree between *Dunkirk* and St. *Sauvier* being larger by about 64 toiſes than between St. *Sauvier* and *Collioure*; and to reduce them to an equality, there muſt be a greater alteration made in the ſituation of thoſe three places, than is reaſonable to ſuppoſe their obſervations are capable of admitting. Here follows the compariſon.

Dunkirk and *Collioure*	a mean degree is	57061
Dunkirk and *Paris*		56960
Paris and *Collioure*		57097
Dunkirk and St. *Sauvier*		57090,4
St. *Sauvier* and *Collioure*		57026,5

According to M. *Picard*. 57060

To conclude, Dr. *Deſaguliers* propoſes a method of obſerving the ſhadow of the earth in lunar eclipſes; whereby the

the differences between the diameters in the oblong ſpheroidical figure, if there be ſuch an one, as M. *Caſſini* affirms (*viz.* of 96 to 95) may be diſcovered.

Let P Æ P Æ (Fig. 17.) repreſent the earth, ſeen from the ſun at the time of the ſummer ſolſtice; it is evident, that the ſame figure will expreſs the ſection of the earth's ſhadow at the moon's diſtance, as ſeen from the earth. If E E repreſent the ecliptic, Æ Æ will be the ſhorteſt diameter of the ſection; and if L L be taken for the path of the moon, in a total and central eclipſe thereof, by obſerving the time ſpent in the paſſage of the center of the moon, thro' the ſhadow, and reducing that time to ſeconds of a degree of a great circle of the heavens, we ſhall have the leaſt diameter of the ſhadow.

Again let the ſame letters (as Fig. 18.) repreſent the ſame things; only here the ſection of the ſhadow is ſuch, as the earth will caſt at the equinox, and the eclipſe of the moon is here ſuppos'd partial, its center juſt touching the ſhadow. When the moon's center is got to *c*, if the latitude of its center or its diſtance from the ecliptic be obſerv'd, we ſhall have the length *c* C nearly equal to the longeſt ſemi-diameter of the ſhadow.

Now comparing *c* C (Fig. 18.) to L C (Fig. 17.) the difference between *c* C and C P in the former figure and between C L and C Æ in the latter not being worth notice; they ought to be to each other, as 96 to 95, which in ſuch a ſhadow will give a difference of about 25″ at a medium, ſenſible enough to be obſerv'd, notwithſtanding the *penumbra*. If therefore thoſe aſtronomers, who have inſtruments nice enough, and ſufficient ſkill in the management of them, to take angles to three or four ſeconds of a degree, will obſerve what the Dr. has been mentioning in total and partial eclipſes of the moon; by ſuch obſervations they will eaſily convince us, that the figure of the earth is ſuch, as M. *Caſſini* ſuppoſes it, or convince him that he has been miſtaken.

The ſemi-diameter of the earth's ſhadow, when the earth is in *perihelio*, and the moon in *apogæo* is 38′ or 2280″, without conſidering the increaſe of the ſhadow, on account of the earth's atmoſphere, which would make it 39′ or 2340″ (allowing one ſecond for a mile) and the ſame ſemi-diameter of the ſhadow, when the earth is in *aphelio*, and the moon in *perigæo* is 46′ 20″ or 2780″, which increas'd on account of the earth's atmoſphere, will bring it to 47′ 20″, or 2840″. Now

if

the ratio of 95 to 96 be taken in both cases you will have these analogies. $\left\{\begin{matrix} 95 : 96 :: 2340'' : 2364'',6 \\ 95 : 96 :: 2840'' : 2869'',8 \end{matrix}\right\}$ So that $2364'',6 - 2340'' = 24'',6$ will be the difference of the semi-diameters, when the section of the shadow is the least; and $2869'',8 - 2840 = 29'',8$ will be the difference of the semi-diameters, when the section of the shadow is the greatest; the sum of those differences $24'',6 + 29'',8$ halv'd, will give the difference, when the section of the shadow is at a medium $= 27'',4$: From which if we take $2'',4$, because in Fig. 18. Cc is a little less than CP, and in Fig. 17. LC is something greater than ÆC; we shall have Cc in Fig. 18. to compare with LC in Fig. 17. which will exceed it by $25''$, if M, *Cassini*'s figure of the earth be the true one.

The End of the SEVENTH VOLUME.

THE

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CORRIGENDA.

PAGE 355 *l.* 24, for adulcorated *r.* edulcorated. *p.* 398 *l.* 24 for this *r.* thus. *p.* 436 *l.* 33. for *crainum r. cransum. p.* 417 *l.* 40 for bolt head *r.* long neck. *ibid. l.* 42 for lid *r.* stopple. *p.* 418 for bolt-head *r.* long neck. *ibid. l.* 15. for decripitates *r.* decrepitates. *p.* 439 *l.* 10 for may *r.* might.